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INTELLECTUAL FREEDOM

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Other Papers Read at the
Autumn General Meeting of the Society
November 4, 5, 1949

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INTELLECTUAL FREEDOM

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CHANGING ISSUES IN ACADEMIC FREEDOM IN THE UNITED STATES TODAY

HARRY D. GIDEONSE

President, Brooklyn College

(*Read November 4, 1949, in the Symposium on Intellectual Freedom*)

THE changing issues in academic freedom discussed in this paper are related to three new developments: (A) the problem of teachers who accept political discipline in matters of science and scientific opinion, (B) the impact on scholarly activities of federal security regulations especially in the field of the physical sciences, and (C) the changing sources of academic financial support.

A. POLITICAL DISCIPLINE IN MATTERS OF SCIENCE

As a by-product of the so-called "cold war" the problems that arise in connection with teachers who accept Communist Party discipline have immediate urgency. The present discussion is conducted as we stand on a quicksand of verbal, as well as factual, confusion in public opinion itself and even in academic circles.

The changing issues in academic freedom are perhaps most easily characterized with the bald statement that several professors at the University of Washington have recently been dismissed on the ground that they were guilty as members of the Communist Party of violating the principles of academic freedom, and therefore of "conduct unbecoming a teacher." [1] The classical formulations of the principles of academic freedom by the American Association of University Professors contemplated no such development. Neither the 1925 nor the 1940 "statement of principles" [2] foresaw a hazard to academic freedom in the ideological commitments of the teachers themselves. They speak of the university or the college as the party that "may not place any restraint upon the teacher's freedom of investigation" or "impose any limitation upon the teacher's freedom in the exposition of his own subject in the classroom or in addresses and publications outside the college." It is true that the 1940 "statement of principles" postulates that the "common good" of institutions of higher learning depends "upon the free search for truth and its free exposition," and recent experience has demonstrated with tragic emphasis that political organizations as well as the federal government itself may place more effective restraints or limitations upon the teacher's freedom of investigation or his freedom of exposition than were ever contemplated by colleges or universities themselves.

The struggle for academic freedom takes a new form as the intellectual, ideological, and cultural landscape changes. The essence of the tradition must be restated and redefined by every generation in the light of current facts, current challenges, and current abuses. Those who live in an atmosphere that is comparatively static will abide by the received verbal formulations. Those who live in a climate of opinion deeply affected by new developments—such as, say, in the modern scene, a university with a real experience with communist infiltration or a large number of teachers in the physical sciences affected by federal security regulations—will find little help in a parrot-like repetition of the old words and phrases. If they are to be a living chart—and not an empty verbal shell covering an alien content—their meaning must be hammered out anew on the anvil of experience. In such a period of restatement and redefinition, it is inevitable that there will be an interval of confusion and even chaos arising from the differences in thought and emotion evoked by old and new verbal symbols, and only a process of sharing and analyzing the new experience will contribute to a basic clarification of the issues involved.[3]

[1] *Communism and academic freedom, the record of the tenure cases at the University of Washington,* Seattle, Univ. of Washington Press, 1949.

[2] Reprinted in *Bull. Amer. Assoc. Univ. Prof.* **35** (1): 66–72, 1949.

[3] There has been considerable evidence of professional rethinking of the fundamentals in current literature. I single out as exceptionally useful: Sidney Hook, Should communists be permitted to teach? *The New York Times Magazine,* February 22, 1949; Alexander Meiklejohn, Should communists be allowed to teach? *The New York Times Magazine,* March 27, 1949; John L. Childs, Communists and the right to teach, *The Nation* (N. Y.), February 26, 1949; Sidney Hook, What shall we do about communist teachers? *The Saturday Evening Post,* September 10, 1949; Sidney Hook, Academic integrity and academic freedom, *Commentary,* October, 1949; and a

This conviction leads me to introduce specific illustrations throughout this discussion since a great deal of the heat of the contemporary debate tends to subside once we are all agreed upon the actual data of contemporary experience. Too many college teachers without any actual experience with communist infiltration continue to discuss genuine cases of identification with party discipline as if they were the usual case of describing a non-conformist or philosophic Marxist as a "red" or a "communist," and nothing in this paper is designed to obscure the fact that there are today, no doubt, plenty of orthodox academic freedom issues in which a teacher is dismissed because he addresses a Wallace-for-President meeting, or in which a trustee interested in dairy farming argues that a campus supporter of the removal of fiscal handicaps for oleomargarine is "promoting communism." These are vulgar cases of reactionary constraint and they should be resisted with all the vigor the academic profession can mobilize. It is precisely the purpose of this paper, however, to argue that we are likely to promote a tidal wave of such misinformed efforts to curb freedom of thought and teaching if we do not vigorously clarify the real meaning of academic freedom in a period when modern totalitarianism itself regards every academic discipline as a potential tool in the capture of power. It is not argued that the number of communists in academic life is so large that they constitute a serious hazard in themselves, but it is argued that the effort to protect teachers who accept outside dictation in their academic activities with academic freedom strengthens the appeal of conventional enemies of intellectual freedom and diminishes the resilience with which the normal sources of defense respond to their challenge. Putting academic freedom to the greatest possible strain is not the most effective method of preserving it.

The function of a university is the discovery and the dissemination of the truth in all branches of learning—and as such the freedom of the academic teacher is essential to the preservation of free society itself. As a social tradition academic freedom is morally anchored in this dedication to truth. For the academic teacher—says Paulsen in a classic passage of *The German Universities and University Study*—"there can be no prescribed and no proscribed thoughts." A member of the Communist Party, however, accepts party discipline in intellectual as well as political matters. His views are not the fruit of an individual scholar's rational analysis of experience. They are prescribed as well as proscribed. They are a matter of the party's "iron discipline," changing every day with the shifts in the party-line, and his membership will cease promptly if he "deviates" in any essential point. It is necessary to stress that this is not a matter of the individual's political *beliefs* but a question of deliberate *action* through the acceptance of disciplined submission to the dictation of the party. There are no part-time or casual members of the party. They are all pledged to active participation in the "vigilant and firm" defense of the party line to insure "the triumph of Soviet power in the United States." An article on the role of the teacher in *The Communist* (May, 1937) says:

> Communist teachers must take advantage of their positions, without exposing themselves, to give their students to the best of their ability working-class education. . . . Only when teachers have really mastered Marxism-Leninism will they be able skillfully to inject it into their teaching at the least risk of exposure and at the same time conduct struggles around the schools in a truly Bolshevik manner.

In commenting on this paragraph Arthur O. Lovejoy, one of the founders of the American Association of University Professors, says in *The American Scholar:*

> In short, a Communist teacher in a school or university may be expected to be in fact, first and last and all the time, a *secret* propagandist and an *indefatigable intriguer* in the interest of the one cause to which he is devoted. Such persons are hardly ideal members of teaching bodies.[4]

These things are true not merely of the American communist. They are characteristic of the entire movement throughout the world. The following paragraph from a recent essay by Harold Laski may serve as a succinct summary by an author whose record is full of evidence of sympathetic observation of the movement he so bitterly characterizes:

> To this there must be added the grave issues created by the ethical behaviour of the Communist Parties outside Russia after 1917. The passion for conspiracy, the need for deception, the centralized and autocratic commands, the contempt for fair play, the

symposium on Communism and academic freedom in *The American scholar*, Summer, 1949, with contributions by President Raymond B. Allen and three dismissed members of his faculty, as well as articles by Arthur O. Lovejoy, Max Lerner, Helen Lynd, and T. V. Smith.

[4] Lovejoy, *op. cit.*, 335.

willingness to use lying and treachery to discredit an opponent or to secure some desired end, complete dishonesty in the presentation of facts, the habit of regarding temporary success as justifying any measure, the hysterical invective by which they sought to destroy the character of anyone who disagreed with them; these, in the context of an idolization of leaders who might, the day after, be mercilessly attacked as the incarnation of evil, have been the normal behaviour of Communists all over the world.[5]

In the light of these facts which are based upon sad experience throughout the world, it is disturbing to observe the thick smokescreen of innocent as well as deliberately misleading comment which obscures the real nature of the adversaries who are locked in battle. Critical comment from those who confuse "communism" with all other types of unorthodox thought can be taken for granted without further citations, and I therefore single out discussion by "liberal" observers since my principal interest is centered in the confusion of vital issues by the use of opaque language with the ensuing danger of weakening support for academic freedom from sources which would normally be the chief reliance of friends of freedom. Thus, an informed observer like Max Lerner can say of the University of Washington cases that "the fact of belonging to a party which is meant to be a disciplined army, and the added fact of having kept it secret for a period, ought to be a weighty item in any calculation about a teacher's integrity." In spite of this consideration, Lerner does not regard such evidence as "clinching" because some of the teacher's students testified "that he always made his bias clear to his students" although the same individual admittedly concealed his membership in the party from his colleagues, and Lerner then argues that non-conformism is part of "the great tradition of our people" because "dissent sharpens the search for the viable truth."[6] But how much non-conformism or dissent is there in the concealment of your heresy? How can concealed dissent contribute to the search for viable truth if party discipline compels you to lie? Another good example can be found in the use that is sometimes made of President Conant's statements in defense of intellectual freedom because some of his readers have not carefully read his qualifications. In a criticism of the University of Washington cases, I find Arthur M. Schlesinger, Jr. quoting Conant's annual report for 1947 as follows:

> The criteria for joining a community of scholars are in some ways unique. They are not to be confused with the requirements of a Federal bureau. For example, I can imagine a naive scientist or a philosopher with strong loyalties to the advancement of civilization and the unity of the world who would be a questionable asset to a government department charged with negotiations with other nations; the same man, on the other hand, because of his professional competence might be extremely valuable to a university.[7]

Clearly, Conant is right and he is well within the classical tradition of academic freedom. What has this quotation to do with the defense of concealed members of the Communist Party at the University of Washington who were engaged in secret activities, assumed "party" names and accepted party discipline? And did Schlesinger entirely overlook the sentence at the bottom of the *same* page of President Conant's report in which he says: "Granted honesty, sincerity and ability, there must be tolerance of a wide diversity of opinion." In other words, Conant is saying that intellectual freedom must be protected *granted* the very qualities which Communist Party members do not possess by definition (because they accept party discipline in these matters). Is this a quotation which can be used to attack a university administration which bases its action upon the disappearance of the very qualities which President Conant takes for granted?

An indication that a large percentage of the current discussion would cease if the facts of the problem were more clearly understood can be found in the views of President Harold Taylor of Sarah Lawrence College, who—like many other liberal educators—believes that communists should not be excluded because if we begin by "excluding communists, we will end by excluding any one who says anything provocative, unorthodox or interesting." The relevant query in reply is: Do party liners *say* anything provocative, unorthodox, or interesting? Or do they rather conceal their identity and, in the language of *The Communist*, inject their views "at the least risk of exposure"? Dr. Taylor's lack of experience with the real problem is illustrated by his defense of the presence of communists on teaching faculties with the plea that democratic education should provide a "daily en-

[5] Laski, Harold J., Introduction to *Communist Manifesto, Socialist landmark*, 89–90, London, Allen & Unwin, 1948.

[6] Lerner, *op. cit.*, 339.

[7] Schlesinger, Arthur M., Jr., The right to loathsome ideas, *Sat. Rev. of Lit.*, May 14, 1949.

counter with truth" that is "free and open."[8] Such a daily encounter that is free and open is precisely the thing that is impossible with members of a conspiratorial, camouflaged group that have accepted party discipline in intellectual matters.

It would be easy to enumerate other illustrations of the extent to which even academic discussion permits terms to be used with a lack of precision, discrimination, and factual documentation that differs shockingly from the nicety with which the same individuals would use terms in their own professional subject matter. Outside academic circles, we find correspondingly opaque use of language, such as that of a recent judicial opinion on a bill imposing penalties on members of the Communist Party in which the Court, following traditional American use of words such as "member" and "party," argued that "members" of a "party" do not necessarily support its full program. In connection with members of the Communist Party, these are questions of fact and in the abundance of contemporary evidence they are relatively easy to document—and such a statement does not necessarily mean that the bill in question was wise or unwise, constitutional or unconstitutional. It does mean that it is no longer intellectually defensible to assume that the key words in the relevant discussion convey the same meaning that they traditionally imply in a free society.

The statement that "membership" in the Communist Party is like "membership" in any other legal party is utterly misleading. To be a registered Republican or Democratic voter—which is presumably the meaning of "membership" in such a case—does not imply the assumption of iron discipline in executing the party leaders' orders even in intellectual matters as politically remote as biology, the history of philosophy, or the theory of music, whereas "membership" in the Communist Party really means that the individual becomes the *agent* of the party—this is the phrase Lenin always used to describe "members"—in every field in which it is seeking instruments to achieve its power purpose. It is also misleading to speak of party membership as "guilt by association" since this would be true of membership in the free society's sense of the term whereas membership in the Stalinist sense means a dedicated commitment to accept the discipline of the group, *which is active cooperation and not association.*

The moral prerequisites of freedom are usually not "spelled out." We take them for granted, but the present situation in free society calls for their explicit statement if vital distinctions in the defense of freedom against its old and its new enemies are to be clearly made. Classically, freedom is always defended as the best method of pursuing the search for truth in which we assume that the truth of tomorrow may be the heresy of today, and the constructive potentiality of heresy is therefore assumed as well as the agreement that each is searching for the truth whatever our differences may be as to its exact nature. It is vital to recognize that the heart of the argument lies in the moral assumption that all of us are dedicated to the truth wherever our individual conscience and insight may lead us to recognize it. Traditionally, we assume that an individual taking a minority point of view sees truth in an unconventional way and is courageously taking the hazard of testing unpopular positions in open controversy on the merits of the argument.

Another cause of confusion in evaluating the changing issues in academic freedom is the persistent tendency to confuse issues of academic freedom and issues of civil liberties. In a current report of the American Civil Liberties Union, "academic freedom" is discussed under the general heading of "Civil Liberties of Teachers and Students."[9] It would seem wiser to preserve some of our traditional verbal distinctions. The term "civil liberties" describes freedoms which are enumerated in constitutional provisions which assure the citizen against governmental limitations or restrictions. There is no "civil liberty" that assures any Tom, Dick or Harry the right to speak or teach in any college or university, or any magazine or newspaper the right to be included in the library of a public high school, and "academic freedom" does not—in any historic formulation by academically responsible bodies—assure any group of students the right to organize on any campus, or any individual the right to teach on that campus irrespective of his character, competence, or philosophical views. In fact, some of our most significant "freedoms" in the American type of pluralist and associational society are anchored in the right of a college or church to *deny* to any individual the right to speak or teach under its auspices, and it is a characteristic tendency of modern totali-

[8] See article in *Town Meeting* **14** (44) : 4–5, Town Hall, Inc., New York.

[9] *Civil liberties of teachers and students, academic freedom, a statement of principles,* American Civil Liberties Union, New York, 1949.

tarians to prescribe uniform behavior in that respect for all institutions, and to *confuse such uniformity with freedom.*

The essential principle was stated in characteristically crisp style by Justice Holmes in an early Massachusetts decision: "The petitioner may have a constitutional right to talk politics but he has no constitutional right to be a policeman." In other words, a man has a constitutional right—a "civil liberty"—to speak freely as a citizen. Under our present law he can "talk" communism, fascism, etc. but there is no "civil liberty" that requires the Department of State or a given college or university to hire him. In spite of the clarity of our law and tradition in this field, we find an intelligent and informed student of our freedoms such as Arthur M. Schlesinger, Jr., discussing the University of Washington dismissals in the light of the "clear and present danger" rule of the Supreme Court in civil liberties cases, leading to the final conclusion that the "monstrous fallacy" of declaring membership on the faculty incompatible with membership in the Communist Party "puts the whole Washington proceedings wholly outside our civil liberties tradition." The statement in essentially irresponsible and uninformed. We do not anticipate or prejudge the final outcome of professional deliberation or meditation upon the University of Washington cases if we recognize that there are some basic issues here that deserve more sympathetic and careful attention from a friend of freedom. No one at the University of Washington ever claimed that the decision had anything to do with civil liberties. They did claim that members of the Communist Party have no right to protection under the spurious claim of academic freedom for those who are pledged to destroy it. The argument should be met as it was stated, and not evaded by a reference to the rights of citizens as they confront their government. The intellectual confusion is deepened with Schlesinger's citation of Brandeis' statement that "only an emergency can justify repression."[10] The Brandeis quotation is, of course, from another Supreme Court decision in a civil liberties case. These references to Holmes and Brandeis are useful reminders of the fact that freedom never has been an absolute. They are, however, relevant to the rights of men as citizens and not as scholars. They have nothing whatsoever to do with a university's right to say that it deems membership in a political group which assumes the right to dictate in scholarly matters incompatible with the responsibilities of free scholarship, or in the words of the President of the University of Washington that "academic freedom means not alone the right to hold unpopular views but the obligation to hold views which, shaped by the accumulation of tested evidence, are subject to no dictation from outside the mind of the holder."[11]

Most of the confusion in our current controversy about academic freedom and civil liberties is embodied in the refusal to admit that today there is no agreement, in fact, on the moral basis of our civil liberties. Justice Douglas of the United States Supreme Court recently made an eloquent plea for the maintenance of civil liberties, in which he made the classical assumption that unpopular minorities consisted of people like John Peter Altgeld, who "do not stand mute when their conscience urges them to speak out." The trouble with our contemporary so-called subversives is, of course, precisely that they consider the truth "a bourgeois prejudice," that they will—as Lenin taught—"use any ruse, cunning, unlawful method, evasion, and concealment of the truth" to help to hasten the triumph of their cause, and that they frequently stand mute when their particular type of conscience orders them to follow the party-line rather than the dictates of the truth in the light of the argument and the evidence.

The same classic assumptions can be found in Justice Holmes' statement in his dissent in the Abrams case that "the best test of *truth* is the power of the thought to get itself accepted in the *competition* of the market." The phrase "competion of the market," of course, calls for open avowal of the minority point of view, and the phrase, "test of truth," again implies a willingness to follow the evidence and the argument wherever it may lead. The real problem with the modern Stalinist is concealment of purpose, camouflaged vocabulary, and avoidance of competition. Holmes' reliance upon the competition of the market as a test of truth is simply irrelevant in dealing with modern communists. Most of us have no fear at all of the test of truth in an open argument with Stalinists who believe in neither truth nor competition, but we are deeply concerned with the consequences of protecting liars and conspirators with academic freedom and civil liberties which were intended as a safeguard for minority points of view that were morally dedicated to a

[10] Schlesinger, *op. cit.* See also Merritt E. Benson's reply in the same journal for September 10, 1949.

[11] *Communism and academic freedom*, 19.

search for truth in an open comparison and competition with conflicting ideas.

Recently, the Committee on Academic Freedom of the American Civil Liberties Union took action on student activities in our colleges which is almost a textbook example of the chaotic emotionalism that now passes for thought in this area. The resolution had two parts. In the first, they laid down the principle that any organization for political action, including the Communist Party, should be allowed to organize on any campus. The second part of the action went on to say that if such a group, after being chartered, was proved to have lied about its affiliations, such findings should be published on the campus, but they should not be ground for suspension *or any other disciplinary action*. Now it is a clearly established principle that an educational institution is supposed to draw some disciplinary conclusion if a student group lies or disregards campus rules, but here the conclusion clearly means that communists have the right to lie without incurring the usual penalties for such behavior. In other words, the American Civil Liberties Union is trying to impose a uniform rule of *moral indifference to untruthfulness* on educational institutions. The conventional answer to such a statement mimics Pilate's "What is truth?" It is admittedly rather hard to determine "the" truth in many cases. A college, however, need not inquire as to the truth of, say, the program of the American Youth for Democracy or the American Student Union, to mention two well-known "transmission belts" for students. When a college charters a Newman Club, it does not pass on the "truth" of Catholic Christianity any more than it certifies to the truth of the Republican or Democratic platforms when the college charters a Republican or Democratic student group. But it is easy to determine whether the organizers of such groups tell the truth about their sponsors, about their finances, about their national affiliations, and when they lie about these things it may be perfectly defensible liberal doctrine and sound educational procedure to say that they should not be chartered. To say that the college should be morally indifferent to such untruthfulness is to promote the disintegration of free society, which the American Civil Liberties Union is presumably organized to defend.[12]

[12] The American Civil Liberties Union excludes members of the Communist Party from its own Council because it has discovered that sincere defense of civil liberties cannot be expected from men and women who do not themselves believe in them. The organization defends the right of communists to a place on the faculty of educational institutions (concerned with academic freedom) since in its judgment membership in the Communist Party has "nothing to do with professional fitness" although in the Union's own judgment such membership disqualifies them from participation in the defense of civil liberties. Here, again, the facts seem to be the issue because Mr. Roger Baldwin, the Director of the American Civil Liberties Union, does not regard a teacher's membership in the Communist Party as an "association" which might "affect" his teaching. Why assume that people who cannot be trusted in the defense of civil liberties are fit to serve on the faculties of free colleges and universities even if they define truth as that which serves the interests of one party and one class? (See *Town Meeting* **14** (44) : 7.)

Let us examine another specific case in which all the data are a matter of record. In a recent Albany decision of the Acting Commissioner of Education, the Board of Higher Education of the City of New York was ordered to reinstate with back pay a teacher who had been dismissed after full and open trial by a unanimous Board under the statutory charge of "conduct unbecoming a teacher." The Commissioner based his decision on the argument that membership in the Communist Party was not a ground for dismissal. The ruling missed the entire point in the case because the Board of Higher Education ruling on the principle in April of 1941—a ruling to which the Acting Commissioner refers but which he does not quote—stated explicitly that it was the Board's intention "to adhere to its established policy not to discharge any member of its staffs merely because of membership in a political organization." The formal legal charges in the case made it very plain that the teacher was dismissed because credible witnesses established to the Board's satisfaction that the individual in question was a liar in an educational investigation conducted under the auspices of the State Legislature, which is the ultimate authority in control of public education, and untruthfulness is still—until further totalitarian revision of our professional standards—"conduct unbecoming a teacher," as defined in the law. It so happens that the untruthfulness was established in connection with the individual's presence at a Communist Party meeting. If the untruthfulness had been established in connection with his attendance of a service in a Catholic Church, it would still be untruthfulness and not proof that he had been dismissed because he attended a meeting under religious auspices.

The issue here was: Should teachers be truthful? The Albany ruling seems to mean that we have amended the Bill of Rights to say that

whereas all other citizens will be punished for perjury, totalitarians will be granted a special exemption. Can we really expect free institutions to endure under such a perverted conception of the moral basis of our liberties? It may be very well to say as a matter of principle that our freedoms should be extended to any one, even to a citizen of totalitarian political sympathies if all other standards of moral and civic decency are observed, but the sober fact of the matter is that any one who has embraced a totalitarian party line has also automatically adopted a form of human conduct which is itself—by definition—subversive of the standards of a free society dedicated to truth. It is never a matter merely of unconventional political *belief*. It is always as an unavoidable by-product of the party discipline accepted by members of such groups, a matter of *conduct* and *action* subversive of the moral basis of free society. I find myself in complete accord with the New School for Social Research which has a by-law stating that "no member of the faculty may be a member of any political party or group which presumes to dictate in matters of science or scientific opinion." Such a policy seems to me a courageous and realistic translation in twentieth-century terms of the essential content of the classical ideal of academic freedom, which is morally rooted in the acceptance of the ideal of truth as the evidence and your conscience may teach you to see it, and not as a party or ideological discipline may force you to twist it.

It is quite unnecessary—and futile—to attempt to remedy the situation created in public education in New York State by the recent Albany ruling to which I referred by new legislation forbidding the employment of communists or other totalitarians. Such legislation may or may not be wise or desirable, but it has nothing whatsoever to do with the principles involved in the Albany ruling, or in the underlying decision of the Board of Higher Education. The Acting Commissioner in Albany referred to the political rights of communists, to the defendant's service in the armed forces, and even to the Bridges case, which—like the flowers that bloom in the spring—had nothing to do with the case. In passing he referred to the detailed testimony concerning the truthfulness of the defendant which was presented by witnesses whose credibility was repeatedly tested and accepted by the Board of Higher Education (as well as by a Legislative Committee designated by both houses of the State Legislature), and without any further investigation or examination of the quality of this testimony—which he did not hear—he merely stated that the defendant denied that he was present at the meetings described by the other witnesses, that he (the Acting Commissioner) was not convinced that he was present on these occasions, and continued his essentially irrelevant dissertation on civic rights. What we need in this specific instance is a vigorous reminder to our public administrators that unconventional ideological belief is no excuse for lying and perjury. In other words—incredible as it may sound—we need to make it clear that a communist can be dismissed for the same type of "conduct unbecoming a teacher" which would lead to the dismissal of a teacher who happened to be a Democrat or a Republican. If the present ruling were allowed to stand, it would lead to a situation in which public administrators would have to insist on lower standards of integrity and decency for communists than for all other citizens, and in which—as a consequence—any type of misconduct would become acceptable if the person in question claimed immunity as a communist from the customary standards of professional honesty and conduct. This is not a denial of the constitutional rights of communists, but the exact opposite, to wit, a demand that they should have the *same* rights—and *nothing more*.

Every case involving concealed party membership with its evasive and devious techniques arouses a storm of protest identifying the "martyrs of freedom" with all the classical cases of the history of freedom. Thus Helen M. Lynd speaks of the dismissal of the teachers at the University of Washington as "a threat to every teacher who values scholarship and responsible teaching above conformity" although the facts in the case illustrate complete conformity with the intellectual dictates of a totalitarian political group. Max Lerner speaks of the "canker of terror" that we have placed in the hearts of a "potential Galileo or Darwin, a Jefferson or Einstein" because we have "placed political limits on teachers" when the facts, of course, are that the University of Washington insisted on a clarification of a scholar's responsibility for independent judgment in the face of an effort to discipline scholarship in the service of a totalitarian society.[13] Similar confusion concerning the basic issues and principles is characteristic of every current issue of this type in academic or in trade union circles.

The cry of academic freedom was of course heard in connection with every stage of the Rapp-

[13] The Lynd and Lerner articles can be found in the symposium on Communism and academic freedom, cited above.

Coudert investigation, although it was professionally encouraging to note that the American Association of University Professors, which had large chapters in the colleges that were involved, never entered the case. Committees of fellow travelers idealized the "victims of the repression of intellectual freedom" as "winter soldiers," as idealists who sacrificed their careers to the truth even if it should be unpopular, and comparisons were made with famous incidents in the repression of minority opinion in the past. If we take a glance back at such incidents as the episode involving the sociologist Ross at Stanford, the economists Nearing and Patten at Pennsylvania, and Robert M. Lovett in the disgraceful Walgreen episode at Chicago, we find in each and every case a man of courage who—whatever we may think of the nature of his thought—had the honesty of conviction to assert his views publicly and to take personal responsibility for his utterances. None of these characteristics emerged in any of the Rapp-Coudert cases.

Since the facts are almost an exact parallel of those that are now emerging elsewhere, and it seems to be part of the therapy of freedom to pass through a set series of stages of illusion and disabusement, there may be some point in a reference to a student performance of James Thurber's well-known play, *The Male Animal,* which I attended in the midst of the Rapp-Coudert investigation. Some parts of the audience apparently read a parallel to the current investigation into the play. although even the most superficial acquaintance with the facts would, of course, have made it evident that the cases were almost exact opposites. In the play the young professor got into difficulty with his college authorities because he intended to read to his class from Vanzetti's letter written while in prison awaiting the death penalty. The young scholar, stunned by the challenge to his integrity as a teacher, not only did not lie about the matter but carried his convictions in an aboveboard, direct, and even provocative manner to members of the board of trustees of his college.

In the play: courage, honesty of conviction, almost reckless willingness to take the responsibility for an unconventional point of view. In the Rapp-Coudert cases: denial of unorthodox conduct and thought, cowardice, perjury, and complete refusal to take the responsibility for one's actions.

Personally, I had almost been hopeful that some man or woman of courage would emerge in the Rapp-Coudert hearings who would arise and assert his right as a citizen and as a scholar to a communist point of view. It might be argued that such conduct would have endangered his position but—in view of the fact that no such cases had arisen so far—that would have to be ironed out in the actual trial hearings and in the inevitable subsequent legal test. Such a case would have received my prayerful consideration and it would probably have been a case in which the accused member of the staff would have received the support of my office. Such cases did not in fact arise. Every single case tried involved clear defiance of the authority of the Board, perjury, anonymous and scurrilous libel of colleagues, or other conduct clearly unbecoming a member of the staff. Not a single case was based upon open and frank admission of unorthodox political and social views. Every single case was typical of a party that has enriched our political vocabulary with words like "party line," "fellow traveler," "transmission belt," "innocents front," "party name" and other terms indicative of devious procedures. The reason for the absence of frank and open admission of unorthodox ideas was undoubtedly the character of the Communist Party which would not permit independent thought or action on the part of its members. They must, by definition, follow the party line, that is to say, they must resign themselves to being intellectual and moral "yes men," and they therefore are almost inevitably involved in conduct unbecoming a member of a college faculty, because their party has no use for members who respect truth for its own sake and who would pursue it even when it conflicted with the day-to-day fluctuations in the instructions from headquarters. In other words: the totalitarian character of the Communist Party organization (or of Fascist organization, if such cases should arise) makes it highly improbable that a case will arise where a member of the staff will be accused *merely* of membership in the Party. Inevitably—because of the character of these anti-democratic groups—such membership will involve the individual concerned in other activities which by themselves are clearly conduct unbecoming a member of a college or university staff.

It is no answer to say that we must overlook moral niceties in the evaluation of the lunatic fringe on the left because of the nature of the emergency or because of the hysteria which characterizes some of the lunatic fringe on the right.

Clearly, there are some thoroughly absurd—and even dangerous—things being said by those who, "throwing the baby out with the bathwater," would save the country from totalitarianism by

suspending the Bill of Rights. Some months ago, one of my colleagues at Brooklyn College invited Mr. J. Parnell Thomas, then Chairman of the House Committee on Un-American Activities, to participate in a college radio program on the topic, "Who and What Is Un-American?" Mr. Thomas said that he didn't like the subject, that it was indecent and shameful for a college to discuss such a subject, that words like American and un-American did not require definition because everybody knew what they meant, that we didn't have the right to discuss it, and when my colleague urged that the Bill of Rights gave us the privilege of discussing any subject as long as it wasn't obscene, Mr. Thomas replied that he didn't care about the Bill of Rights. Most of us are quite ready to grant that a tired public man may sometimes be harassed into the use of careless language, but we may still be profoundly disturbed about a gentleman assigned to the task of applying the yardstick of true Americanism to the conduct and behavior of his fellow-citizens, who regards the discussion of his yardstick as shameful or indecent. In fact, in my judgment—and, I feel happily certain, in the judgment of the overwhelming majority of my fellow-citizens—such a statement is a perfect example of un-American activity. The fact that there are some folks who abuse their freedoms—of whose conduct we may disapprove as firmly as any one in the Congress—does not change our point of view. It is quite possible to believe that there is a legitimate place for a Congressional Committee on Un-American Activities, and to regret that its present personnel and procedures are frequently amateurish, ignorant, and lacking in the qualities that would make its work fruitful.

We are in fact in a period in which totalitarian and conspiratorial groups are using academic freedom and civil liberties as a cloak to cover moral indecency totally subversive of the ideals of liberty and truth, while some of the critics of these groups are themselves utterly ignorant of the nature of the institutions they presume to defend. While the fringes grow and are increasingly irresponsible, it is time to strengthen the center by a vigorous reexamination of our basic concepts in the light of current experience.

We can all agree with President Conant's recent statement that panic may lead government to submit teachers to a disastrous interference with freedom of teaching and of thought. In a free society we need to preserve the process of arriving at reasoned convictions which emerge from diversity of opinions, honestly expressed and freely argued. There are—as Whitehead has said—in every generation those who must carry the cross uphill, and there suffer for the next generation. Whitehead did not say that they could do that while they pretended that the cross was an umbrella, or the painful climb upward a pleasant descent into the valley. There is something obscene in attempts to justify the misconduct of a conspiratorial group of perjurers and liars by references to John Peter Altgeld—or even to Socrates. Socrates—and Altgeld—took before the whole world the full consequences of their own passionate deviotion to the truth. These are sacred things to a free society, and they do not mean that liars should be protected against the penalty for untruthfulness, or that conspiratorial and anonymous libel of colleagues should be tolerated in the name of the secular saints of liberty and reason.

B. ACADEMIC FREEDOM AND FEDERAL SECURITY REGULATIONS

The second cause of changing issues in academic freedom, that is to say, the impact on scholarly activity of federal security regulations, has not received attention in public or academic discussion corresponding to the significance of the restraints that have been imposed on free science ostensibly in defense of free society. We do not have to search far for the reasons. "Security regulations" are in their nature not publicized and individuals who experience their administrative application are themselves rarely in a good position to discuss them. Even informed opinion is rarely aware of the nature of the cultural conditions in which free science has prospered in the past, and the restraints imposed on scientists are accompanied by such a vast expansion of financial aid that a critical evaluation of administrative policies seems to run contrary to the patent facts of public support.

Science has been—and will continue to be—a major factor in the defense of an open society. There is today an enormous public interest in the physical and biological sciences. It expresses itself in public appropriations that may even be in excess of the research and creative potentialities that are immediately available. It springs from admiration of the role played by science in World War II, and from concern about the role science may play in a possible World War III. It is part and parcel of the fear of sabotage and espionage, and it expresses itself in the elaboration and enforcement of a variety of "security" provisions.

Some of these provisions or procedures attract public attention, such as the stories in the *New York Herald Tribune* about the scientist whose "loyalty" was questioned because his landlord found a copy of *The New Masses* in the family garbage (which could happen even to a rabid "red baiter" with some slight concern for documentation), or his colleague who was deemed unfit for national service because he had "a relative who was married to a communist" (which could happen in the most patriotic family). Others do not attract public attention, but their experience is part of the atmosphere or climate of opinion in which scientists work. In the present Cold War, concern about sabotage and espionage is certainly legitimate. Concern about procedures designed to protect the national interest in science that may "throw the baby out with the bathwater" is equally legitimate—and, in my judgment, vastly more urgent.

We live in an age of fear and propaganda. Some fifteen years ago a great President reminded us that we have nothing to fear but fear itself. Today we live in a climate of opinion in which the drive for security—born of fear and propaganda—has become almost hysterical, and in which we forget that we have just come victoriously out of a war with totalitarianism because a free society can mobilize resources that have been destroyed in closed societies by totalitarian practice.

Fear could lead an open society like our own to destroy its most precious assets in a struggle with a closed society of the Nazi or Soviet type. This is not beautiful theory or wishful thinking, but the very essence of our own experience in the recent war. The Germans were engaged in atomic research, and German science was no negligible competitor. Atomic research happens to be in a highly theoretical field in which Americans have not normally been interested in the past because of our drive to applied science and technology. But our doors and windows were wide open rather than closed. In accordance with the principles of an open society, we took men and ideas from all over the world, and as ideas from Italy, Hungary, France, Scandinavia, and even Germany played into the customary open and aboveboard process of scientific interchange and reciprocal criticism and enrichment, we ended up with the ideas and the technical execution before the other side did. *We did not achieve leadership because we observed rigorous security regulations.* The opposite is true. We led because we followed the true principles of an open society and encouraged quite consciously an open process of exchange of ideas. Most of the ideas came from abroad, and many of the men were citizens of other countries or even refugees from our own enemies in the war. Security was imposed in the final stages during the war, but we won our leadership because we followed the principles of free society and free science—and our enemies lost because essential men and vital ideas did not get a full chance to play into their own research programs. S. A. Goudsmit, an American physicist who was officially assigned to the study of the scientific war effort in Germany, has summarized his conclusion as follows:

> Too many of us still assume that totalitarianism gets things done where democracy only fumbles along, and that certainly in those branches of science contributing directly to the war effort the Nazis were able to cut all corners and proceed with ruthless and matchless efficiency. Nothing could be further from the truth. . . . The failure of German nuclear physics can in large measure be attributed to the totalitarian climate in which it lived. There are lessons we can all learn from that failure.[14]

It is vitally urgent to make no mistakes about these matters now. From my own observation of the security regulations now in effect, I doubt whether we are getting the benefit of the strength of our own position. It is one thing to mobilize scientists in wartime when every one is ready to give patriotic service. It is quite another thing to impose controls in peacetime on a professional group which has a tradition of achievement that calls for the exact opposite. The details of these controls and a more extensive analysis of their full impact on intellectual freedom can be confidently left to the authors of other papers in this program. Their impact on academic freedom is sharp—both in teaching and in research.

Modern wars are won by "big" industry, backed by "big" laboratories and "big" science. No one who reads James P. Baxter's official history of the role of science in our war effort—*Scientists against Time*[15]—can fail to be aware of the crucial importance of the relationship, or to overlook the extent to which it was the "open" and competitive character of our scientific life that contributed vital ideas from all over the world. I have already stressed the international interplay of men and ideas that made the atomic bomb possible. Perhaps I should not forget the vital role of Alexander

[14] Goudsmit, S. A., *Alsos*, The search for the German atom bomb with an examination of the failure of German science, xi, London, Sigma Books, 1947.

[15] Boston, Little, Brown and Co., 1947.

Sachs' intellectual salesmanship in persuading President Roosevelt that a gamble of several hundred million dollars might be rewarding—the fact that an economist for a New York banking firm could know as much as he did of recent scientific developments is itself due to the climate of opinion of a free and open society which lends itself to such relational cross-fertilization in scholarship, industrial applications, and public service.

The record of our scientists in the war effort is vaguely appreciated by the general public. There is, however, a dangerous tendency to relate scientific activity almost wholly to its military usefulness and there is little understanding of the type of cultural conditions in which science flourishes, and almost no conception at all of the extent to which a continuous flow of achievement depends upon a constant and open process of interplay of ideas, experiments, and discussion. Those who talk about closing doors and keeping secrets—one Brooklyn patriot has even proposed that we "should keep our borders closed, coming and going, to domestic and foreign professors"—should ask themselves who would have had the key ideas in atomic fission if foreign professors had been kept out of the United States. Do these patriotic obscurantists realize that radar and penicillin came from England, DDT from Switzerland and Germany? What would be achieved by preventing the Library of Congress from publishing its *List of Russian Accessions*—as seriously proposed by congressional statesmen from the Republican (Taber) as well as Democratic (Rankin) side—except that our scholars and scientists might be kept ignorant of significant Russian contributions?

Security does not spring from control and regulation. It comes to a free society because the open exchange of new truth and knowledge affords a leadership that in a world of incredibly rapid change can spring only from a rate of progress and growth which keeps one step ahead of the "secure" and "regulated" conditions of the police state. Scientific information is like a fish that has been caught—it doesn't improve by efforts to keep it well preserved. We grow in science by casting our nets out anew all the time. Our true security lies in the maintenance of this process of casting out the net again and again, and it does not lie in preserving the "security" of the fish that were caught yesterday. The totalitarians had a theory that a state that was "totally" prepared for war would inevitably conquer the "unprepared" democracies. But the record shows that such early "total" preparation froze the economic and technical structure in a stage of development that is inferior to that of a society in which a large section of economic, scientific, and technical life remained open to experiment, exchange, and discussion. It is this same society in which, after mobilization, a vast array of new ideas and practices gave a resilience to an eventual war effort that would not have been available if "total" mobilization had been ordered in an earlier stage of preparedness to provide national "security."

There are, therefore, two conditions which a free society must meet if it is to maintain the leadership in science which we now enjoy. The first is the preservation of the conditions which have given us that leadership in the first place. The second is the development of a form of administrative organization that will preserve professional incentives and establish working conditions which will insure the quality of public service in scientific work.

If the government wants to secure the services of the ablest scientific personnel, it must arrange for employment conditions that evoke a creative response on the part of first-rate men. President Conant has recently stressed the fact that ten second-rate men cannot take the place of the one first-rate,man in this field. If we need the services of the cream of the profession, we need employment conditions that will attract them. When scientists observe the treatment received by men like Lilienthal and Condon in scientifically illiterate congressional committees, they draw certain conclusions as to their own desire to submit to comparable abuse. The congressional assumption that you can overcome such difficulties by appropriating large sums of money for research ignores the fact that high scientific ability cannot be purchased like lard or pig iron, but must work under conditions that evoke a maximum use of creative potentialities.[16]

Ours is not a totalitarian world in which men can be ordered around from trade to trade. We would not think of selecting a Secretary of Agriculture or of Labor who was personally unacceptable to farmers or to organized labor. When will our public men learn that the quality of scientific achievement depends upon the selection of men who can evoke a creative response from professional people? Are we, in a spurious pursuit of security, going to throw the greatest resources of an open society away because we fear the opera-

[16] Civil liberties of scientists, *Science*, 177–179, Aug. 10, 1949.

tion of the very principles that have given us leadership?

In a world of dangerous national and ideological competition, there is no security in provisions that make for secrecy about *past* achievements. The only security that is worth while lies in the *preservation of the conditions that have given us leadership thus far,* and those conditions call for open and creative interchange of all the true talent, irrespective of race, creed, or national origin. The way to defend an *open* society is to keep it *open*—let's leave the closed doors to folks who believe in them as a matter of principle. The history of free science demonstrates abundantly that those who lock the doors to the laboratories are likely to lock out more than they lock in.

C. CHANGING SOURCES OF ACADEMIC FINANCIAL SUPPORT

The third cause of changing issues in academic freedom discussed in this paper, viz. the changing sources of academic financial support, has almost completely escaped public discussion although few questions have been canvassed as comprehensively in academic circles. A college president approaches the topic with due diffidence in a period in which the public demand for higher education presents such shocking contrasts with the volume of its financial support. There are some issues here, however, which affect the core of the tradition of academic freedom, and although the interrelations are often subtle and indirect, a pattern of economic support is developing which affects the quality of academic personnel as well as the direction in which its creative talents are utilized. In a sense this is simply a new form of an old challenge—although it is not generally recognized as such—because the "jurisprudence of academic freedom" abounds with citations of cases in which a donor sought to limit a college's teaching or selection of students or faculty personnel by such stipulations as required the teaching of the "fallacies of socialism" or the "defense of free enterprise," and we are all reasonably familiar with the difficulties inherent in the donors' terms concerning certain types of religious bequests.

The shrinking bases of private financial support in fees or in endowment—which typically permitted considerable professional discretion in determining the priorities of academic expenditure in teaching as well as in research—and the shift to support by foundations and by a variety of government agencies have given these issues a new form which creates new hazards to free scholarship. If these dangers are clearly recognized, we shall probably be able to cope with them as effectively as we have in the past dealt with similar subversive characteristics of private support. British experience with the University Grants Committee suggests that a democratic government is quite compatible with a degree of professional autonomy in the expenditure of public funds which would seem almost utopian at the moment in the United States, and my plea at this stage is for intensive study of current practice, including the typical stress on progress through the emulation of the "best existing practice" which has always been such a positive force where freedom and organization are combined in a fruitful and constructive fashion.

The subtle and indirect forces are far more worthy of study than the more vulgar and direct issues. Practical experience has made us all familiar with the restrictive influences that may arise in economics, biology, or chemistry if a large "dairy" bloc in a legislature seeks to protect itself against the competing claims of oleomargarine or other butter substitutes, and corresponding issues in the freedom of social scientists have left their mark in connection with conflicting pressures on budgetary agencies from manufacturers' or trade union groups. Those who pay the piper and want to call the tune are clearly visible in such cases, and while the characters in the drama have changed, most of us can see that the intellectual issues raised by wealthy donors or religious groups in the past (and in the present) are roughly similar to the pressures now exerted by farmers or trade union groups.

It is more difficult to see that when government sources—either directly or indirectly—support a very large volume of historical research, paying salaries far in excess of current academic levels, they materially affect academic priorities and prestige, and deflect creative and professional talent into the study of problems and "fields" which hardly express the professional views of historians concerning the relative value and promise of alternative projects which may not command public support. Shifting fiscal trends are therefore compelling some of the best of our creative talent to devote their most fruitful years to work selected by government agencies rather than by scholars themselves, and corresponding hazards ensue as to qualitative standards in the remainder of scholarly activity. It is difficult to persuade an indi-

vidual scholar who warmly welcomes the handsome increase in his meager academic salary that his case is an illustration of a new and subtle hazard to academic freedom, but the conclusion seems unavoidable that the tendency *as a whole* has such implications, which are more dangerous precisely because they are not recognized as such in individual cases. We do not have to accept the vulgar insistence that the government will seek to dictate the conclusions of such fiscally-supported research. Evidence on the whole does not bear out such a view, but the selection of problems for creative talent—scarce at the best—*precludes* the selection of other "free" projects, and in the long run freedom may be more seriously—and subtly—endangered by selection than by outright censorship or prohibition.

Similar tendencies are inherent in the second category of perils to academic freedom discussed earlier in this paper. "Defense" related research means the selection of research projects by defense agencies, and it appears to be true that our government has been singularly willing to support basic as well as applied research projects. Here, too, subtle and indirect forces are at work. A handsomely financed federal project leads to the availability of expensive equipment—and the development of human skills and aptitudes—which themselves have an influence on the determination and selection of "free" projects, and the relation of this possibility to the modification of the forces which have been the greatest single strength of free science is clear to any student of the cultural setting in which free science has flourished in the past. In fact, some of the data available to me suggests that federal support of such research has gone so far already that more basic research may be supported today by federal fiscal resources than by all the academic sources in the country added together. Doesn't the data suggest the need for some comprehensive survey of the facts by an academic group interested in the *general direction* of academic trends rather than in the specific projects that are at present supported? Isn't it at least possible that some general policies may be stated which would protect the values of free scholarship and supply a framework for the discussion and settlement of specific issues?

In the social sciences these new fiscal influences exercise a negative rather than a positive influence. It is clear that the vigorous promotion by federal subsidies of research in the physical and biological sciences—and of personnel training programs in those fields—may widen the gap that already threatens the stability of our society between scientific and technical change on the one hand, and the relative inflexibility of the moral, social, and legal framework of society, on the other hand. It is equally clear that more intensive study of techniques of social adjustment, including the possibility of an imaginative and constructive program in the humanities, might contribute to the possibility of minimum assurance of stability. The various social sciences may have a great deal to contribute in this field but we are politically unprepared for the acceptance of these moral and intellectual possibilities. The same legislators who press for large federal subsidies for additional physical and technical research—thereby contributing to the strength of the causes clearly working towards unbalance—refuse to appropriate funds to study the social and economic by-products of the new developments in the physical sciences. Our legislators find it very hard to distinguish between the study of social change on the one hand, and the advocacy of social change on the other. Any one who has had experience in the legislative process knows how easily social science and collectivist propaganda are confused. If a large share of the resources supporting the physical and biological sciences is to come from federal sources in the future, academic groups may have a special responsibility to insure that the social and humanistic studies do not shrink to proportions that will themselves constitute a major hazard to the future of free society, and therefore to the "freedom" even of the fiscally more privileged physical scientists.

This paper has offered a hasty but suggestive survey of some of the most urgent of emerging issues affecting the position of free scholarship in our open society. The treatment has not been "objective" and value judgments have been freely expressed. Free men and women cannot afford to be neutral when the survival of free society itself may be at stake. The defense of freedom will not come from minds that are "so open there's nothing left but a draft." The present struggle for freedom does not call for objectivity but for reasoned prejudice—prejudice in favor of the conditions in which freedom can continue to flourish and to grow. No pretense is made that the issues have been discussed comprehensively and it is hoped that a sharp formulation may stimulate further discussion and inquiry concerning relevant facts and policies.

Freedom is first of all the availability of choice,

and the survival of freedom calls for an intellectual understanding of the choice as well as for the development of a moral will to choose. A free community depends upon the strength and vitality of its shared values. Historically, freedom emerges when internal checks can be substituted for external constraint, and—conversely—freedom is endangered if a free community's shared values are no longer sufficiently vigorous to create the moral cohesion on which the discipline of free men rests. When you pulverize a rock, you have dust. When it rains on dust, you have mud. That—in brief—is the problem of a free community anchored in shared values when it is tested in vital challenge by a totalitarian enemy after it has undergone continuous erosion in a purely analytical period of secularization. That is how a free democracy may become what Walt Whitman called a society of men with hearts of rags and souls of chalk. The only remedy is a resolute breed of men—resolved to think anew the moral conditions of freedom and determined to act on the fruit of their thought.

SOVIET RESEARCH IN THE SOCIAL FIELD

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(*Read November 4, 1949, in the Symposium on Intellectual Freedom*)

For reasons which are familiar, work in the social sciences is doubly accessible and vulnerable to criticism by laymen, in comparison with that of the natural and biological sciences. In so far as social scientists are engaged in analyzing and interpreting, or sometimes merely in commenting upon, events and processes in the society around them, they cannot escape entirely from the assumptions and presuppositions common to their own times and peoples. Similarly, the points of departure or arrival of their trains of thought may be of considerable consequence to their fellow-citizens.[1]

True, if social scientists state their processes of thought and their conclusions in esoteric phrase, as psychologists and economists so often do when conversing with one another, the eavesdropping layman is likely to tiptoe away somewhat mystified. In so far as the social scientist deals with the data lying about him in our society and speaks in plain words about matters requiring action, he must expect reactions, mild or fierce, from his fellow-citizens. The real question is the way in which lay holders of power express their reactions to the activities of social scientists. Here the question of the dispersion or concentration of power is of capital importance.

SOCIAL SCIENCE AND THE SOVIET STATE

In Soviet society the position of social scientists differs greatly from that of their American counterparts. Soviet scholars also work in a society in which they and their product are judged by laymen. But judgment is rendered by busy administrators who have no time to be practitioners of the sciences. The lay judges are not individuals, struggling to be heard against a cacophony of divergent opinions. They are the supreme leaders of a monolithic Party. This Party claims to possess a monopoly of wisdom, embodied at any given time in the selected texts of Marx, Engels, Lenin, and Stalin.

Intellectual monopoly is in turn exercised through an administrative monopoly of power, which can be used to regulate the welfare and life of each individual. Thus the role of the lay "consumer" of social science research, which is dispersed widely through American society is, in Soviet society, concentrated in a few men, possessed of absolute power. Yet these men are laymen concerned basically with the uses of power within the Soviet Union and with the world-wide Messianic role of the Soviet state.

Since the emergence of the Soviet regime its leadership has been deeply concerned with the content of the social sciences, as well as of literature, music, and the arts. After an initial period of toleration, or, perhaps, of neglect, the heavily charged atmosphere of the First Five-Year Plan (1927–1932) brought with it a tightened control and direction of the sciences and the arts. A new period of relative toleration (1933–1936) was followed by the wild self-immolation of the period of the purges.[2]

The war with Germany, with its appeal to humane revulsion from Nazi barbarities and pseudo-scientific stupidities and its final rehabilitation of Russia's national past, brought with it a widening of the range of "permitted" thought, provided, naturally, that it contributed its part to the upsurge of the patriotic will to self-defense and victory. Many observers, in Russia and abroad, believed that victory in the war, in alliance with Great Britain and America, would strengthen and preserve the enlarged sphere of intellectual elbow-room and would allow Soviet scholarship quietly to reenter the general community of learning. As one Soviet writer said to me in 1943 with deep conviction, "The regime *must* trust the people now, for the people have saved the regime."

[1] *Cf.* Becker, Carl L., Everyman his own historian, in his book of the same title, 233–255, N. Y., Crofts, 1935.

[2] *Cf.* Mosely, P. E., Freedom of artistic expression and scientific inquiry in Russia, *Annals Amer. Acad. Pol. and Soc. Sci.* **200**: 254–274, 1938.

POSTWAR RETURN TO INTELLECTUAL AUTARCHY

Even before the end of the war the shift back to an intensified "Bolshevist vigilance" was under way. The system of party schools, in which local party leaders and propagandists are trained to transmit the will of the Party to Soviet society, was notably strengthened in November 1944. Instead of, as previously, ascribing the victory to "the Russian people," this merit was now reserved to the party, alone acclaimed henceforth as "the organizer of victory." The idea of a community of thought with Russia's democratic allies in the victory, which was referred to with some cordiality as late as 1945, was now repressed forcibly.

In view of the political direction of the social sciences, as well as of art and literature, it is in the pronouncements of political leaders that the key to the postwar trend must be sought. Two such landmarks are Marshal Stalin's election speech of February 9, 1946, and the late Andrei Zhdanov's address to the Moscow Union of Writers, on September 17, 1946.[3] Marshall Stalin's speech laid special stress on the theme that the war had proven the absolute superiority of the Soviet system over all others. By implication it emphasized the revival of Soviet autarchy, in cultural as well as in political life.

Zhdanov's speech to the writers of Moscow was mainly directed against elements of irony, lyricism and similarly individualist and apolitical manifestations of Soviet literature. In it only one paragraph foreshadowed an all-out attack on "bourgeois" influence in Soviet intellectual life. This paragraph is worth citing, however, since it gave the keynote to the turmoil of the years which have followed:

> It is no accident that in the literary journals of Leningrad people have been carried away with enthusiasm for the contemporary, low-caliber bourgeois literature of the West. Some of our literary people have begun to regard themselves not as teachers but as pupils of bourgeois and petty-bourgeois literati, have fallen into a tone of kow-towing and self-prostration before petty-bourgeois foreign literature. Is such kow-towing fitting for us, Soviet patriots, for us who have built the Soviet order which is a hundred times loftier and better than any bourgeois order? Is kow-towing before the narrow-minded petty-bourgeois literature of the West fitting for our progressive Soviet literature, the most revolutionary literature in the world?

[3] A. Zhdanov, Doklad o zhurnalakh "Zvezda" i "Leningrad," *Literaturnaia gazeta*, **39** (2302) : 2–3, 1946.

ATTACK ON SOVIET ECONOMISTS

On this call to self-sufficiency and self-isolation the changes have been rung ever since September 1946. After the scourge had swept through literature and music, it descended upon the economists. The chief target was Eugene Varga, long a leading figure of Soviet economics, editor for many years of *World Economy and World Politics* and Director of the Institute of World Economics in Moscow. At a series of official conferences, held in Moscow on May 7, 14, and 21, 1947, Varga's study of *Changes in the Economy of Capitalism Resulting from the Second World War* was subjected to severe criticism. The author was accused of neglecting political factors of power in favor of "objective" economic factors, of asserting that the bourgeois state is capable of regulating the economy for a general, classless or non-class purpose, and of stating that colonies could be liberated from metropolitan control without a revolutionary struggle. Above all, Varga was accused of ignoring the "general crisis" of capitalism, the precursor of its eagerly anticipated downfall.[4]

It is significant that the Varga controversy was not carried to the point of eliminating Varga himself from the field of economic study. Personal factors may have played a role, although thirty years of work as a prominent party-member do not necessarily provide immunity. Varga's Institute was merged with the Institute of Economics, headed by Ostrovitianov, his relentless critic.

Ostrovitianov and the work of his Institute have also been subjected to severe criticisms. *Culture and Life*, which rivals the *Literary Gazette* in pointing out the shortcomings of Soviet scholars, made a slashing attack on the Ostrovitianov Institute in its issue of August 31, 1949:

> . . . Of late the Institute has produced few works summing up economic processes going on in the Soviet and the foreign economy. Economists do not

[4] Stenographic transcript of the discussion of Varga's book, on May 7, 14, and 21, 1947, approved for publication on December 8, 1947, published as a supplement to the November 1947 issue of *Mirovoe khoziaistvo i mirovaia politika;* available in English translation as *Soviet Views on the Post-War World Economy*, Washington, 1948, Public Affairs Press in cooperation with the Russian Translation Program of the American Council of Learned Societies. See also Frederick C. Barghoorn, The Varga discussion and its significance, *Amer. Slavic and East European Rev.*, **7** (3) : 214–236, 1948; and Ruth Amende Rosa, The Soviet theory of "People's Democracy," *World Politics*, **1** (4) : 489–510, 1949.

devote the necessary attention to working out such very important questions of Soviet economics as expanded socialist reproduction, the organization and payment of labor, the disposition of productive forces, cost accounting, lowering of prime costs, accumulations in excess of plan, acceleration of turnover of working capital, price formation in the Soviet economy, questions of mass production and the formation and utilization of state reserves.

In the Institute, questions of the economy and politics of the people's democracies are poorly examined, changes are not analyzed which took place after the second world war in industry and agriculture in the capitalist countries, and processes connected with the advent of the economic crisis in capitalist countries are not studied.

The Institute of Economics does not fulfill its plan of research work from year to year. For example, out of nineteen works laid down by the 1948 plan only two works were accepted by the directors of the Institute. . . . The Institute of Economics and its director, Comrade Ostrovitianov, are under obligation to reorganize their work in Bolshevist fashion. . . .[5]

ATTACK ON SOVIET PHILOSOPHERS

The wave of repudiation of "Western" thought spread in 1947 to the field of philosophy. Georgii Aleksandrov had emerged before and during the war (he is now forty-one) as the leading academic philosopher and as chairman of the Party's key Committee on Propaganda. His one-volume *History of Western European Philosophy,* published in 1946, was widely acclaimed and received the accolade of a Stalin prize. However, at a conference held on June 24, 1947 at the Institute of Philosophy of the Academy of Sciences, Zhdanov turned the barbs of his criticism against Aleksandrov's book. Among his criticisms were those of underestimating the originality of Marxism in relation to earlier German philosophies, a condescending attitude towards Russian philosophers of the nineteenth century, a failure to imbue his work with "party-spirit."[6]

Concerning almost all older philosophers Comrade Aleksandrov finds occasion to say a kind word. The greater a bourgeois philosopher, the more incense is offered up to him. All this results in Comrade Aleksandrov, possibly without himself suspecting it, falling into captivity to the bourgeois historians of philosophy, whose starting-point is to see in each philosopher an ally in the profession and only then to see him as an adversary. If such conceptions were allowed to develop among us, they would inevitably result in objectivism, in kow-towing before the bourgeois philosophers, in an exaggeration of their merits, in depriving our philosophy of its militant offensive spirit. That would mean departing from the basic principle of materialism—its sense of direction, its party-spirit.[7]

Aleksandrov was removed from his post as Chairman of the Party's Committee on Propaganda, but was appointed Director of the Institute of Philosophy.

Zhdanov's role in defining the post-war trends in Soviet social science was further clarified in his speech of September 1947, delivered at a conference held in Poland, at which the Communist Information Bureau, or Cominform, was organized. In his analysis of "aggressive" American policy and "peaceful" Soviet policy Zhdanov accuses the United States of seeking to rob country after country of its independence, of a "design to enslave Europe," of maintaining the largest army in the world, of "hatching a new imperialist war." According to Zhdanov,

> The war-mongers realize that long ideological preparation is necessary before they can get their soldiers to fight the Soviet Union. . . .
>
> One of the lines taken by the ideological campaign that goes hand in hand with the plans for the enslavement of Europe is an attack on the principle of national sovereignty. . . .[8]

ATTACK ON SOVIET JURISPRUDENCE

The Soviet conception of "sovereignty" has been spelled out by Dr. I. D. Levin in a recent issue of *Soviet State and Law.* According to Levin, the exercise of national sovereignty "presupposes the formation of a sovereign national state or of a sovereign multinational state based on the voluntary union of equal nations." Soviet foreign policy . . . "is a model of a consistent policy of re-

[5] Overcoming shortcomings in the work of the Institute of Economics, *Kul'tura i zhizn',* 3, August 31, 1949; cited from the summary published in the *Current Digest of the Soviet Press* **1** (35): 12–14, 1949, with slight adjustment in the translation.

[6] Zhdanov, A. A., *Vystuplenie na diskussii po knige G. F. Aleksandrova "Istoriia zapadnoevropeiskoi filosofii," passim,* Moscow, 1947. *Cf.* varying treatments of this episode by Percy E. Corbett, The Aleksandrov story, *World Politics* **1** (2): 161–174, 1949; and J. and M. Miller, Zhdanov's speech to the philosophers: an essay in interpretation, *Soviet Studies,* **1** (1): 40–51, 1949.

[7] Zhdanov, A. A., *op. cit.,* 19.

[8] Zhdanov, A. A., *The international situation; Speech delivered at the Informatory Conference of representatives of a number of Communist Parties held in Poland in the latter part of September 1947,* 27, 29, 31, Moscow, 1947.

spect for the sovereignty of all peoples and states, large or small. . . ." On the other hand, "imperialist cosmopolitanism is the worst enemy of the self-determination and freedom of peoples . . . ."[9]

The attack on "bourgeois cosmopolitanism" in the field of jurisprudence has been pressed forward steadily. The task of Soviet jurists is not an easy one. As Dr. V. Chkhvikvadze wrote in the *Literary Gazette* of September 7 of this year,

> . . . it must be recorded that until now the decree of the Central Committee of the Communist Party concerning preparation of textbooks on such important subjects as the theory of state and law, the history of the Soviet state and law, and the history of political science and bourgeois public law has not been carried out. The U.S.S.R. Ministry of Higher Education has ordered the textbooks *The Criminal Trial* and *Soviet Public Law* excluded from the list of recommended literature because they abound in serious mistakes. However, many of the textbooks which the students of juridical higher educational institutions will have to use likewise contain many serious shortcomings. . . .
>
> The task of Soviet jurists [he concludes] is to show the profound ideological decay experienced by bourgeois juridical science, to show that there is no branch of bourgeois juridical science which might not fall under the baneful influence of imperialist reaction.[10]

THE "CLASSLESS SOCIETY" AND "COSMOPOLITANISM"

What is the source of this intensified demand for intellectual autarchy? Of course, a period of "cold war" may have its intellectual casualties anywhere. Since the Soviet government is engaged in a desperate struggle to enlarge the area of its domination, a period of extreme tension in intellectual life, of extreme demands for "party-spirit," is also to be expected.

The source of the tension goes far deeper, however. According to the Soviet philosophy, the dialectic of history has already destroyed "classes as such" as the moving forces in Soviet society. It might be thought that the establishment of an officially classless society—which may be proclaimed any month now—would lead to a relaxation of tensions and of controls within that society. This view, which is a derivative from the notion of the "withering away of the state," is, of course, heretical in the Soviet Union today, and has been heretical there since the purge of Soviet political science in 1937.

The present-day Stalinist analysis holds, on the contrary, that the closer Soviet society approaches to the stage of Communism, the more the agencies of compulsion and coercion must be strengthened, that the greater the "success" of Soviet power the greater is the danger of attack by a coalition of hostile states. Having allegedly eliminated all class conflicts within its own society, danger can henceforth come only from without. In the political field the dangers are attributed to the United States, its chief rival in the polarization of power. In the intellectual field the main danger is the influence which foreign "bourgeois" and "cosmopolitan" science, art, and letters might exert upon Soviet intellectuals.

Domestically, there is a complete denial of any internal Soviet conflicts of interest which might provide the soil for the slightest deviation from pursuit of the tasks set by the state and Party. Any failure to achieve the goals set is now attributed to foreign machinations or to influences or suggestions emanating from the "bourgeois" world. In the intellectual field deviations from the goals set by the Party-state are attributed to the influence of "bourgeois pseudo-science." Vulnerability to foreign suggestions is called "cosmopolitanism." As the breast-beating historians proclaimed earlier this year,

> Serv ity and obsequiousness toward bourgeois science—in any manner and any form—are particularly ruinous for historians of modern and recent times; servility toward bourgeois science frequently takes the form of objectivism, of rejecting the party and class point of view on historical events and on their interpretations in bourgeois historiography. . . .[11]

ATTACK ON SOVIET ORIENTALISTS

"Misery loves company." The historians, having been castigated for their sins, turned upon the Orientalists. The leading historical journal accused the historians of the East of avoiding the study of recent and contemporary history.

[9] Levin, I. D., On the essence and significance of the principle of sovereignty, *Sovetskoe gosudarstvo i pravo* **6**: 33–46, 1949; cited from condensed text published in *Current Digest of the Soviet Press* **1** (39) : 3–7, *passim*, 1949.

[10] Chkhvikvadze, Dr. V., On major shortcomings in juridical science, *Literaturnaia gazeta*, **72**: 2, 1949; cited from condensed text published in *Current Digest of the Soviet Press*, **1** (39) : 16–18, *passim*, 1949.

[11] Tasks of Soviet historians in the field of modern and recent history, *Voprosy istorii*, **3**: 3–13, 1949; cited from condensed text published in *Current Digest of the Soviet Press*, **1** (35) : 7, 1949.

Soviet Orientology . . . impresses the reader with the almost complete lack of any really timely subject matter in its articles and reviews. For instance, the fifth volume of *Soviet Orientology* (1948) contains ten articles in the "History" section. Two of these are devoted to ancient history, three to the period from the third through the sixth centuries, two to the fourteenth century, two to the eighteenth, and only one to the 1850's. *Soviet Orientology* just did not reach the twentieth century. . . .

We lack comprehensive works expounding as a whole the history of China, Iran, India, Korea, Mongolia and other countries of the East. . . .

American Oriental studies and American bourgeois historians diligently assist the present-day imperialist claimants to world domination, attempting by every means to justify the "right" of the United States of America to a leading role with respect to Eastern countries. It is the direct duty of Soviet historians of the East to expose the true predatory role of American imperialism in the East, to expose by countless historical examples the hypocrisy and perfidy of American aggressive policy, particularly in China, the Philippines, Korea, and the Near East.[12]

ATTACKS ON SOVIET HISTORIOGRAPHY

This extreme emphasis upon the role of history writing as a tool of immediate policy has not always been so pronounced in Soviet social science. In contrast to the present demands upon historians it is refreshing to turn to a review of Soviet historiography, entitled *Twenty-five years of historical science in the U.S.S.R.*, published during the war by the Institute of History of the Academy of Sciences.[13] This collective study, edited by several of the best historians of present-day Russia, passed in review the achievements of Soviet historiography in each field, appraising the principal sources and publications and noting the deficiencies in an impartial way. It noted the superiority of Soviet studies of early Russian history, as well as the almost complete absence of study of the other Slavic countries and of the Soviet period. It was a work of historians written for historians, not for politicians.

Like historians of the East, Soviet historians in general have been found guilty of neglecting modern and recent history. *Culture and Life* has taken them severely to task for this retreat to the past.

As yet the members of the Institute [of History] staff have not written scientific works on the history of the Soviet state, the history of the working class in the U.S.S.R. and the history of the revolutionary movement. . . . The work of exposing Anglo-American imperialism and the reactionary essence of social-reformism is poorly conducted. . . .

The magazine's [*Voprosy istorii*] unsatisfactory treatment of problems of the history of the Soviet state is explained to a great extent by the fact that the editorial board oriented itself on a small group of historians headed by Academician I. Mints, who considers the history of the Soviet state his indefeasible monopoly. Yet it is known that Academician Mints and a number of his supporters, who permitted errors of a cosmopolitan nature, have not written important works in the field of history. . . . *Voprosy istorii* is not conducting an intensive attack against Anglo-American and other bourgeois falsifiers of history . . . .[14]

CONFLICT BETWEEN MONOLITHIC POWER AND FREEDOM OF INQUIRY

The quandary of the historian is no greater, perhaps less, than that of other social scientists in the Soviet Union. The political leadership, which has an absolute power over the livelihood and life of the individual, to a degree hardly imaginable for people who have not lived under a totalitarian system, demands studies of current and actual problems. The scholar flees to the past or to esoteric problems not too closely connected with current policy. The conflict between the demand of policy-makers and the need of the scholar to think out his own answers is always present in latent or active form. At present the conflict is unusually acute, although actual exclusion from scientific work or physical extermination appears to occur infrequently, in comparison with the purges of the 1930's.

Loyalty to the state and to the Party is no protection for the need of the scholar to have room to think. A work of scholarship may receive the highest rewards, academic and material, only to be cast into the limbo within a few years or even a few months. The party line changes, but the

[12] Urgent tasks of Soviet historians of the Orient, *Voprosy istorii*, **4**: 3–8, 1949; cited with slight adaptation in translation, from *Current Digest of the Soviet Press*, **1** (36) : 3–6, *passim*, 1949.

[13] *Dvadsiat' piat let istoricheskoi nauki v SSSR*, edited by V. P. Volgin, E. V. Tarle, and A. M. Pankratova, Moscow-Leningrad, 1942.

[14] For a high ideological and scientific level: concerning the magazine *Voprosy istorii*, by A. Mitin and A. Likholat, *Kul'tura i zhizn'*, April 21, 1949; cited, with slight adjustment of translation, from condensed text published in *Current Digest of the Soviet Press* **1** (26) : 22–23, *passim*, 1949.

written words of the scholar remain. It is no excuse for the Soviet scholar to allege that a given work was in accord with the party line at the time it was published. If the line has subsequently changed the scholar may be punished just as if he had written it under the new dispensation. As one Soviet scholar expressed his predicament, in a private conversation, scientific work is a lottery in which the scholar can never win but can always lose.

Because political power is concentrated in a few hands, power to direct scientific work is also closely held. There is sufficient information available for preparing a sociology of Soviet scholarship. The key figure seems to be the party entrepreneur who is able to interpret the party line to the scholars working under his direction, to secure political "protection" (in Russian, "protektsiia") for the work of his institute, to satisfy some still higher figure in the party leadership of the value of the work of his institute for the Party, and thus to secure political and financial support.

When such a mediator falls from grace, the anatomy of his power is laid bare. When *Culture and Life* turned upon Academician Mints other historians spoke up against him. At a meeting last March of the Learned Council of the Institute of History, Professor Sidorov accused Mints of "servility toward German historiography as far back as 1928." "Somewhat later Academician Mints came out with anti-Party views on problems of the history of our Party." "Academician Mints was able to monopolize the treatment of the history of Soviet society." [15]

In its philosophical claims Marxism aspires to represent a highly rational concept of human society and human progress. It claims to represent the intellectual methodology by means of which man can ultimately escape from the operation of blind forces and take control of his own social destiny. As applied in the Soviet state, scholarship is deprived of access to important materials, and it is forced to acknowledge and "prove" a detailed set of conclusions which are laid down for it by political authority. It is forced to abandon any idea of self-propelling intellectual evolution and exploration and to follow the continual shiftings of the Party line. Under these conditions it is remarkable how many scientific achievements have emerged from the work of Soviet scholars, how much patient labor has gone into answering the questions which the scholar must ask of his data, not of political authority. The Soviet regime needs the work of its social scientists, in order to manage an increasingly complex society, but it rejects, as a matter of philosophical principle and of daily political practice, the spirit of free inquiry under which alone social science can flourish and serve society.

[15] Meeting of the Learned Council of the History Institute of the U.S.S.R. Academy of Sciences, March 24–28, 1949, *Voprosy istorii*, **3**: 152–155, 1949; cited from *Current Digest of the Soviet Press* **1** (35): 9–12, *passim*, 1949.

FREEDOM OF SCIENCE

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(*Read November 4, 1949, in the Symposium on Intellectual Freedom*)

"FREEDOM OF SCIENCE" is gradually becoming a slogan and is losing its sharply defined significance. According to some users, it means "accept no Federal money for education," others see in it a demand for special privileges for professors to behave as eccentrically as they please. It is thus necessary to define, first, our interpretation of this expression.

In this article we shall mean by "science" a search for the relations existing between the phenomena observed in nature and the formulation of a system of these relations which helps us to describe or to "understand" the observations. We, therefore, restrict ourselves to what is often called "fundamental research." This short and incomplete description will have to suffice for our purpose.

What is most striking about the results of "science" is their evident objectivity. This is especially true for the sciences in which the art of experimentation has attained a certain perfection and is the main source of progress such as in physics. The outcome of a well performed experiment is the same regardless of where, or by whom, it is done. Even in the limiting cases where the performance of the experiment influences the phenomenon to be studied, such interference has been recognized and can be properly included in the system describing the phenomena.

The conclusions and deductions drawn from the experiments are usually unique. The system of relations, or the "laws" we derive from them, are common to all scientists. At the frontiers of science it often happens that our experiences are insufficient to allow a single explanation. In such a case, two or more alternate hypotheses will be accepted by all scientists in this field of investigation until further experimentations lead to new discoveries which may decide which of the hypotheses is "correct." It also happens that results of new experiments neither fit existing theories, nor support sufficiently some newly formulated hypothesis. The observations remain then "unexplained" until again later experiments lead to new theories which might include the, at first, unexplained data. In the physical sciences it is comparatively easy to point out such still unexplained facts.

The description of "science" given here, though quite superficial, implies what is meant by its "freedom." Science contains its own means of growth; each new investigation leads to new knowledge and often also to new unknowns. "Freedom of Science" signifies this self-contained development; it means the strict exclusion of outside, non-scientific consideration in the forward trend of fundamental research.

To be more specific, if scientific methods are unable to decide which of several possible hypotheses in physics is correct, the decision should not be made by means of religious, ethical, political, racial, or other non-physical arguments. This seems trivial, but it is unfortunately what has happened in the past and is still happening today.

Religious dogma, interfering with science, has delayed our knowledge of nature by several centuries. Germany, not long ago the most productive country in modern science, saw its fundamental research practically wiped out by the Nazi doctrine. Similarly, Communistic teachings have declared certain scientific discoveries to be opposed to dialectical materialism. The pattern is always the same; it is the decision about scientific matters by non-scientific reasoning. Its effect is always the same too, namely, the frustration of science.

It may be clarifying to notice that the forces which interfere with science have one characteristic in common, their utter inflexibility. One man can adhere to only one such doctrine at a time, whereas a scientist can easily master simultaneously a number of conflicting theories, hoping that future work will remove the conflict. The strength of the democratic form of government, as opposed to totalitarianism, lies perhaps also in its flexibility which it shares with science.

In addition to totalitarian interference, there are other influences which can greatly hamper the progress of science. The advance of fundamental scientific knowledge is entirely independent of its useful applications. In recent decades, however,

the gap between basic research and economically or militarily important applications has become almost non-existent. This has caused an increased pressure on scientists to direct their research along the lines of most urgent applications. Such work is, of course, highly recommendable unless it detracts from the simultaneous development of pure science.

A majority of research is financed nowadays by government agencies and by private industries. The primary interest of these sponsors is obviously the applications of science. The research program is, therefore, directed along certain well defined channels, dictated by non-scientific interests. Such course of action does not lead to the advancement of science, itself.

A well known extreme example is the atomic bomb. It has erroneously been called a scientific achievement mainly because scientists played a major role in its production. However, all the scientific principles used in its construction were known the world over before the war. Moreover, the work on the bomb causes a stagnation in scientific advancement. No new discoveries of any basic significance were made on the bomb project. These had to wait until those same scientists returned to their peacetime laboratories. The main contribution to science made by war research was the development of improved techniques and equipment.

Fundamental science will not necessarily flourish if its main support comes only from those interested in its uses. There are some well known exceptions. Industrial research laboratories have produced a few fundamental discoveries, but this is not the rule; it merely reflects the farsightedness and prosperity of the industries in whose laboratories these discoveries were made.

The too strict control of science will have the eventual result of even defeating the aims of those in need of its applications. Directed research will simply lead to the expansion and improvement of existing methods and will not produce any new ideas. No amount of conventional weapons research would ever have led to an atomic bomb.

What is stated here about military use is, unfortunately, also true for peacetime applications. The possible practical uses of basic science are totally unpredictable and this, in itself, makes its control illogical. We do not know at present which branches of science, whether physics, biology, chemistry, or mathematics, will make a major contribution to the problem of combating cancer. Only through the support of *all* fundamental research can we hope to receive the maximum benefit of the applications of science, as well as an understanding of nature.

Another factor which has greatly hampered science since the war is unnecessary secrecy. Advances in science are primarily due to the exchange and pooling of knowledge gathered by scientists the world over; each individual contributes a little step to its progress. The contribution of some may be primarily in noticing the relations of discoveries made by others. Fortunately, the secrets in atomic science have not yet done much damage because they are shared by a large number of active scientists and mainly involve techniques. Some of the recent important basic discoveries originated in foreign countries where scientists are not hampered by unrealistic secrecy rules. Great care must be taken that no fundamental research is subjected to restrictions. Even the delays imposed by secrecy rules upon the rapid dissemination of scientific results are detrimental to the progress of science.

The French have built an atomic reactor and the Russians have recently even exploded an atomic bomb. This clearly indicates the futility of secrecy in basic science. The Atomic Energy Commission tries to make a distinction between fundamental research and technical "know-how," imposing restrictions primarily upon the latter. There are still a few critics of the Commission whose knowledge of the operation of modern science is so meagre, that they fail to understand this distinction. Some commentators still talk about the atomic bomb "formula" as if it were a baby's diet. They believe that spies gave this "formula" to the Russians who would not have succeeded without it. They overlook that for any scientist the principle of the bomb is as obvious as two and two equals four. We can, fortunately, ignore these misguided critics, since the general public is too intelligent to accept their infantile, distorted views.

It has been frequently said that the creative work of the scientist is very similar to that of the artist. The artist's aim is beauty, the scientist's aim is understanding. There exist applied arts and applied sciences but the creative power is most fertile in fields remote from applications or outside pressures. There is one major difference between creative art and science: the artist can be fairly individualistic and independent, whereas the scientist, on the contrary, can only create new under-

standing by integrating all previous knowledge, gathered by his fellow scientists.

Science can only flourish in an atmosphere in which its value is appreciated. The confusion between science and technology has to be eliminated from the minds of the general public. The stress on applications should be removed from attempts to popularize modern science. It is especially important that beginning students be aware of the importance of pure research. Eventually the financial advantages of a career in applied science over a research position must disappear.

The most productive way to make science grow is, and has always been, teaching. Among the important scientific contributions of many scientists are their pupils. It follows that freedom of science, progress of science, can be realized only if we have freedom of teaching. Teachers must be selected solely on the basis of competence in their field and the art of teaching.

The decline of physics in Germany can be blamed for the greater part on the Nazi-appointed teachers. For example, the great physicist and great teacher Sommerfeld was succeeded by the bigoted, ignorant Müller at the University of Munich. The few sane physicists not dismissed by the Nazis made several vain attempts to educate their many prejudiced Nazi-colleagues, to show them the stupidity of mixing doctrine and physics. Remaining strictly objective and omitting the other evils of Nazism from our considerations, it must be realized that the Nazi beliefs do not interfere as much with applied science as with fundamental research.

Today, the same question arises with regard to Communistic dogmas. Can a fanatic believer in dialectical materialism be a good science teacher or promoter of research? The answer is clearly "no," when his research and teaching is near the frontiers of science, but "yes" when his work deals primarily with applications and generally accepted scientific results. The fact that a scientist is a believer in certain non-scientific dogmas is often, but not always, detrimental to the success of his work. His colleagues are the most competent judges of the value of his research and of whether its results are warped by prejudice.

Occasionally Communist scientists are being discharged by university administrations. There may be several valid reasons for these dismissals, though it is surprising that similar reasons never brought about the firing of any Nazi or Fascist sympathizers. That his Communistic views interfere with his scientific research and teaching should be invoked as a reason for dismissal only when this can be properly substantiated. If every believer in inflexible dogmas were considered unfit for research and teaching, the universities would lose a large fraction of their faculties. It should be noted that anti-evolution laws, anti-vivisection laws, and secrecy restrictions are as harmful to science as totalitarian dogmas.

The study of fundamental science is directly contradictory to the belief in inflexible tenets. If, as often happens, a young man starts out with preconceived ideas, his scientific education will usually remove these prejudices altogether. This is one of many reasons why it is unwise to investigate fellowship applicants for their political beliefs. Awarding the fellowship and furthering their education may be the most effective (and inexpensive) way of leading them towards democratic ideals.

Fanatic adherents to dogmas will ultimately be unable to recognize the objective state of mind essential for an unbiased search for truth. Such men are fortunately rare in science and their work will clearly show the earmarks of distorted logic. They do not belong in research or teaching and deserve no more consideration than the occasional crackpot who tries to crash the world of science with fantastic ideas born in a diseased mind. Thus a free science carries in itself the means of weeding out the undesirables, those whose ideas are in conflict with the ways of science and, therefore, of necessity also in conflict with the progress of humanity.

SCIENCE AND NATIONAL AUTHORITARIANISM

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(*Read November 4, 1949, in the Symposium on Intellectual Freedom*)

THE term national authoritarianism is here broadly interpreted to include laws, regulations, practices, or attitudes of national groups that tend to hamper or suppress freedom of inquiry and expression, toward which we have been striving with varying success for many centuries. Science has been extricated from the entanglements of magic and it appeared to have been largely emancipated from ecclesiastic and political dogmas also. So great had been the progress that Taylor wrote in *The March of Mind* (8, p. 310) :[1]

> The conflict of Science and Authority ended with the Evolutionary Controversy in the 'seventies. Since that time the scientific method of objective observation, experiment and statistical examination has invaded every department of knowledge. Whatever has to be investigated today, is investigated by the scientific method, no matter whether the subject is the mental processes of animals, the optimum conditions for growing beetroot, or the type of chocolate preferred by insurance clerks. A century ago, the approach to these subjects, insofar as they would have commended themselves as matter for investigation at all, would have been the construction of a theory on more or less apriori grounds supported by a few well-known facts. Today the tendency is to discard theory and to collect and classify great numbers of instances and obtain from them some statistical rules.

Published in 1939, this statement summarizes the trend toward the substitution of precise and quantitative investigations for easy generalizations based on insufficient observation and experimentation.

And yet, one of the aims of biology in the middle schools of Russia in 1944 was stated as follows:

> To show how man can act on plants and animals and how, based on the knowledge of the laws of development of the organism, he conquers the nature of plants and animals and what immense prospects open in this direction in influencing the organism in the rearing of plants, animals, fish, and in other branches of agriculture. To show the success of science in the work of Michurin, Lysenko, and scientific investigational institutions of the U.S.S.R. [2, p. 217.]

Quotations from a speech made by T. D. Lysenko before the Russian Academy of Agriculture on July 31, 1948, give an idea of the Michurin-Lysenko-creed (1, p. 132).

> We, the representatives of the Soviet Michurinist trend, contend that inheritance of characters acquired by plants and animals in the process of their development is possible and necessary. . . . Michurin's teaching shows every biologist the way to regulating the nature of plant and animal organisms, of altering them in a direction required for practical purposes by regulating the conditions of life, i.e., by physiological means.

And again,

> Unable to reveal the laws of living nature, the Morganists have to resort to the theory of probabilities, and, since they fail to grasp the concrete content of biological processes, they reduce biological science to mere statistics. [1, p. 136.]

This is the old controversy regarding the inheritance of acquired characters, and, according to Lysenko,

> The Michurinian teaching, which is materialist and dialectical in its essence, proves by facts that such dependence does exist.
>
> The Mendel-Morgan teaching, which is metaphysical and idealist in its essence, denies the existence of such dependence, though it can cite no evidence to prove its point. [6, p. 19.]

The fact is, of course, that there is overwhelming evidence against the theory of inheritance of acquired characters.

That a man of Lysenko's wide experience with crop plants should make such statements is surprising, but almost incredible is the fact that he, an experimentalist, chides himself with not having used his official position sufficiently to suppress the "anti-Michurinist trend." He states (6, p. 29),

[1] Numbers in parentheses indicate references in list at end of article.

But the condition in the Academy has now sharply changed, thanks to the interest taken in it by the Communist Party, the Soviet government, and J. V. Stalin personally.

The interest of the Academy was not purely academic, for on August 26, 1948, its Presidium issued a decree, from which the following excerpts are quoted (1, pp. 138–140):

The session of the Lenin All-Union Academy of Agricultural Sciences raised several very important problems before Soviet biological science, the solution of which should further the great cause of Socialist construction. The session revealed the reactionary, anti-popular nature of the Weismannist-Morganist-Mendelist trend in biological science and exposed its supporters. The smashing of the anti-Michurinist tendency has opened new opportunities for the creative development of all branches of progressive biological science.

The material from the session has shown with all clarity that a struggle between two tendencies, diametrically opposed with regard to their ideological and theoretical positions, has taken place in biological science: a struggle of the progressive, materialistic Michurinist trend against the reactionary, idealistic Weismannist-Morganist trend.

The Michurinist movement, enriching the theory of evolution and revealing the laws of the development of living nature, has, by its methods for the controlled changing of the nature of plants and animals, made an outstanding contribution to the practice of socialist agriculture. The Weismannist-Morganist movement, which declares that changes in the heredity of an organism are independent of the particular conditions of life of the organism, defends idealistic and metaphysical positions, is separated from reality, is disarming the practitioners of agriculture in the matter of improving existing kinds and establishing new kinds of plants and animal species, and engages in fruitless experiments. . . .

If Darwinism in the old form set itself the task of merely explaining the evolutionary process, then the teachings of Michurin, which have received further development in the work of Academician T. D. Lysenko, raise and solve the problem of the controlled alteration of the hereditary properties of plants and animals, raise and solve the problem of guiding the evolutionary process.

T. D. Lysenko, and his followers and students, have made a vital contribution to the Michurinist biological science, to the cause of the development of socialist agriculture, to the cause of the struggle for high yields from agricultural crops and high livestock productivity.

The Presidium of the Academy of Sciences of the USSR obligates the Department of Biological Sciences, biologists, and all natural science research workers, working in the Academy of Sciences, to make a basic reorganization of their work, to occupy a leading place in the struggle against idealistic, reactionary teachings in science, against servility to foreign pseudo-science. The natural scientific institutes of the Academy of Sciences must struggle actively for a further development of the Motherland's biological science and, first of all, for a further development of the science established by Michurin, Dokuchaev, and Vilyams, and extended and developed by Lysenko. . . .

3. To abolish, in the Institute of Cytology, Histology, and Embryology, the laboratory of cytogenetics headed by Associate Member N. P. Dubinin, because it has occupied an anti-scientific position and has proved its unfruitfulness during the course of several years. To close the laboratory of botanical cytology in this same Institute because it follows the same incorrect and anti-scientific tendency. To eliminate the laboratory of phenogenesis in the A. N. Severtsov Institute of Evolutionary Morphology. . . .

6. To review the membership of the scientific councils of the biological institutes and the editorial collegia of the biological magazines, withdraw from membership the supporters of the Weismannist-Morganist genetic theory, and replace them with representatives of the progressive Michurinist biological science.

The misunderstanding or misrepresentation of the miscalled "reactionary, idealistic" genetics and the glorification of the so-called "progressive, materialistic Michurinist trend" might be expected to emanate from impatient men primarily interested in quickly obtaining results of practical value. But the pronouncements are made by the Presidium of the Academy of Sciences of the U.S.S.R., which has wide powers in determining policies and administering funds for research. Moreover, the general condemnation of non-conformists is followed by orders for removal or demotion of individuals and abolition of laboratories because of their alleged "anti-scientific position."

The consequences of the authoritarianism that resolves by ukase differences of opinion regarding facts, principles, philosophies, and procedures must be borne by the Russian people. The substitution of official decree for experimentation in settling differences of scientific opinion can hardly add to intellectual enlightenment; and official prescription of a methodology for plant and animal improvement based largely on a teleological philosophy is unlikely to conduce to sure and rapid progress.

Had this happened a century ago, a modern biologist probably would say, "It could not happen now." But it has happened now, and in Russia,

where biology and its applications were making commendable progress.

Americans are likely to say, "It could not happen here." But it has happened here, and not long ago. The controversy among Russian scientists was not whether evolution is a fact but rather about the facts of evolution, about the way in which evolution comes about. The authoritarian settlement seems amazing in this relatively enlightened age.

We should recall, however, that there raged in the United States of America, not longer than a score of years ago, a controversy as to whether evolution was even a fact. Fundamentalist nonscientists attempted to discredit scientists by debate, ridicule, or mere volume of verbiage; and, unable to prevail by forensics, the anti-evolutionists attempted to prevail by law. Bills to prohibit or restrict the teaching of evolution were introduced into seventeen state legislatures and were enacted into law in two. Though the laws probably did little harm, the world was treated to the grotesque spectacle of the "Dayton monkey trial." It would be unwise to forget that authoritarian methods were used successfully by scientifically-ignorant but strongly-organized groups to suppress or restrict the teaching of scientific facts. And this was only one example of powerful attempts to impose authoritarianism in other fields—history, economics, and sociology. Unorthodoxy became un-Americanism, to be ferreted out and branded with red, white, and blue letters; honest and intelligent criticism often was stigmatized as disloyalty. Perspective was warped; we were myopic, dogmatic, intolerant, and authoritarian.

Authoritarianism jeopardizes scientific progress wherever and whenever it occurs—in United States of Soviet Russia or in United States of America, in the Dark Ages or in the Twentieth Century. It seems incredible that in our generation the law of organic evolution could be amended by decree in United States of Soviet Russia and repealed by statute in two states of United States of America, especially after this enlightening and useful scientific law had been centuries in evolving.

With their relative freedom of thought, many Greek philosophers from Thales to Aristotle developed the concept of natural evolution, largely by shrewd speculation. Aristotle recognized the necessity for induction, the formulation of principles from facts, and his science and philosophy acquired such authority during the Middle Ages as to impede progress because biologists tended to study Aristotle rather than nature. Meanwhile the dogma of special creation and constancy of species arose to delay the development of wisdom from the extensive knowledge that accumulated regarding plants and animals. It required intellectual boldness and moral courage for Descartes to write, even with such evident caution (7, p. 142),

> All the same, if we can imagine a few intelligible and simple principles upon which the stars, and earth, and all the visible world might have been produced (although we well know that it has not been produced in this fashion), we reach a better understanding of the nature of all things. . . .

The better understanding was finally furnished by Darwin and Wallace almost one hundred years ago, when they marshalled the overwhelming evidence which, after almost twenty-five hundred years, finally convinced intelligent people of the fact of evolution, although the course of evolution and the mechanisms of heredity were still either imperfectly or dimly seen.

It remained for Mendel, the Austrian monk; De Vries, the Dutch botanist; Morgan, the American geneticist; and hundreds of others, to show how plants and animals can change or be changed by hybridization and mutation, thus establishing the basis for the science of genetics. Four facts are important in these steps in the founding of this science: (1) principles were derived from exhaustive observations and experiments, not from the authoritarianism of dogma or tradition of individuals or institutions; (2) facts and principles were clearly stated in objective terms, free from mystical, magical, political, or ecclesiastical cobwebs; (3) facts were accepted, conclusions were reached, and principles were formulated as verified truths, regardless of their practical utility; (4) thousands of scientists have verified, amplified, or modified the basic facts and conclusions on which the laws of heredity are based. The only claim that scientists make for the validity of these laws is the degree of universality of their applicability, even though ultimate explanations for some phenomena are still lacking.

Scientists themselves may not be universally free from the taint of authoritarianism, but the whole spirit of science is objectivity; the true scientist accepts facts, whether useful or harmful, whether good or bad. And if he makes a discovery, he expects to have its validity tested many times and in many places by many competent investigators. Dishonesty or incompetence is soon detected and exposed, simply because scientists are continually becoming more precise and critical, are using more

quantitative methods, and because so many of them are working on similar problems that no one of them could fool the others very long. Accordingly, scientific truth will eventually prevail, regardless of the individual, institution, or nation of its origin.

Scientists cannot scientifically be more assertive than the verifiable evidence justifies. They may anticipate nature and make lucky guesses of what is probably true; they may formulate working hypotheses on the basis of limited facts; they may make generalizations limited to the facts available at a given time; but their assertiveness must be limited to what is known. They recognize as truth only what they know to be true, not what they think or hope may be true. They may speculate, but they must distinguish between speculation and knowledge.

Science does or should dedicate itself to the discovery and humanization of truth. Some scientists must concentrate on discovery; others on the utilization of discoveries for human enlightenment or human betterment. In the zeal for human betterment, a disservice sometimes is rendered by charting and initiating action campaigns before science has furnished the necessary guideposts of facts and principles. In general, scientists are less guilty of premature action now than formerly because sad experience has taught them caution.

All this is platitudinous; but it is pertinent to the general subject of this discussion and to the repeal of the laws of genetics and the straitjacketing of the art of plant breeding in Russia. According to Lysenko:

> We, the representatives of the Soviet Michurinian trend, contend that inheritance of characters acquired by plants and animals in the process of their development is possible and necessary. [6, p. 18.]

And, according to the Presidium of the Academy of Sciences,

> The genius of the transformer of nature, I. V. Michurin, created a new epoch in the development of Darwinism. The teachings of I. V. Michurin are based on the great creative force of the Marxist-Leninist philosophy. The Michurinist teachings set as their main task the direction of organic nature and the establishment of new forms of plants and animals, needed by the socialist society. [1, p. 139.]

Here we have it: Soviet science is the defender of Darwinism; Michurinism is based on Marxist-Leninist philosophy; its main task is to breed new forms of plants and animals to improve Soviet agriculture. Its foundation is "the principles of dialectical materialism of the revolutionary transformation of the world in the interests of the people" (Pravda Ed.).

Actually, the scientific philosophy is teleological and its aim is ultra-utilitarian, a dangerous combination that is likely to obscure truth and jeopardize the attainment of practical results.

Lysenko, however, claims that his genetics is useful and the Mendelist-Morganist genetics is practically useless. Can anyone ignore the contributions of the Mendelist-Morganist genetics in the development of hybrid corn, with its record of two billion additional bushels of corn in the United States during World War II; can anyone ignore the rust-resistant varieties of wheat; the superior varieties of potatoes, of flax, of alfalfa, of cotton, of practically all important food and feed crops; and can anyone ignore the improvement in certain domestic animals in United States of America and other countries where the Mendelist-Morganist genetics is the scientific basis for the arts of plant and animal breeding?

Most of Lysenko's claims of practical results from the "New Genetics" are either unconfirmed, or explained on the basis of well-established principles. His vernalization procedure, in which winter wheat seed is given special treatment to make it behave like spring wheat, is effective but of relatively minor value. The claim of "Creative transformation" of species—hard wheat (durum with 28 chromosomes) to several varieties of soft wheat (bread wheat with 42 chromosomes)—by several years of fall planting, in which environment and nutrition presumably donated a new set of chromosomes would be a biological miracle. But here is Lysenko's own statement. [6, p. 47.]

> I am confirmed in this opinion by the data of experiments for the conversion of hard wheat (durum) into soft (vulgare).
>
> Let me note that all systematists admit that these are good, unquestionable, independent species.
>
> We know that there are no true winter forms among hard wheats, and that is why in all sections with a relatively severe winter, hard wheat is cultivated only as a spring, not a winter, crop. Michurinists have mastered a good method of converting spring into winter wheat. It has already been mentioned that many spring wheats have been experimentally converted into winter wheat. But all of those belonged to the species of soft wheat. When experiments were started to convert hard wheat into winter wheat it was found that after two-three-four years of autumn planting (required to turn a spring

into a winter crop) durum becomes vulgare, that is to say, one species is converted into another. Durum, i.e., a hard 28-chromosome wheat, is converted into several varieties of soft 42-chromosome wheat; nor do we, in this case, find any transitional forms between the durum and vulgare species. The conversion of one species into another takes place by a leap.

We thus see that the formation of a new species is prepared by altered vital activity under definite new conditions in a number of generations. . . .

The conversion of a hard wheat into several soft wheats in this manner is hard to understand.

Equally difficult to understand is the insistence that plants can hybridize vegetatively, that grafting may result in altered heredity.

The implication that the egg of a plant chooses to be fertilized by that particular pollen grain that will be best for the progeny imputes to plants both foresight and restraint that even human beings have not developed after long consideration of eugenics.

Lysenko is the apostle of Michurin, who was the "Russian Burbank," who in turn was the American "plant wizard," who was born in 1849 and died in 1926. Burbank stated (4, p. 13):

Mother Nature . . . knew that sameness, monotony, exact re-duplication over and over again would make this world the dullest point of light in the whole universe.

Apparently variation was invented in order that Mother Nature might not be bored!

There is no possible room for doubt that every form of plant life existing on this earth is now being and has always been modified, more or less, by its surroundings.

It is not the duration of an environment that affects heredity; it is the amount of pressure exerted.

Whatever this may mean, Burbank and Lysenko wrote in similar key, because, according to the latter,

It is possible to alter and to create varieties with a desirable heredity by controlling conditions of the external environment, the living conditions of the plant organisms. The heredity is, as it were, the essence of the conditions of the external environment assimilated by the plant organisms in a series of preceding generations. [5, p. 65.]

This is much like Burbank's statement,

My own studies have led me to be assured that heredity is only the sum of all past environment. . . . [3, p. 383.]

It would be a boon if man could rely on plants and animals to absorb from their environment those hereditary characters most useful to them and to society. Man will do better, however, to use his own intelligence rather than trust to that of plants, animals, and environment. Environment does exert an effect, of course, in producing temporary modifications. It also has a selective effect, but evidence does not justify faith that it has a creative effect that transforms plants and animals mysteriously and beneficently into new and useful forms.

Environment may sometimes seem to change hereditary factors. Forty years ago it was thought that wilt-susceptible varieties of flax could be changed into resistant varieties merely by growing them on wilt-infested soil. Resistant varieties actually were obtained, and it appeared as if acquired characters had been inherited. Subsequent investigations showed, however, that a variety of flax may be uniform with respect to flower color and other easily visible characters but may comprise a very diverse population with respect to wilt resistance; the individuals in the population look alike but behave differently toward wilt. When a variety is grown on wilt-infested soil, therefore, the susceptible individuals are eliminated by the disease, the resistant ones survive, and the system works. But the environment has exerted only a selective effect and not a creative one.

Many similar examples of selection for disease resistance could be cited, but the method does not work when there are no resistant plants in the population. Unfortunately, this is true in a distressingly large number of cases.

Selection sometimes is effective in obtaining winter-hardy varieties of crop plants. If one starts with a variety that appears uniform to the eye but comprises both hardy and tender individuals, the tender plants will be eliminated during the winter and the hardy ones will survive, provided the winter conditions are sufficiently severe to kill the tender plants and leave only the hardy ones.

The type of plant selection adopted in Soviet Russia may work if breeders start with a heterogeneous plant population and subject it to conditions which eliminate the unfit and permit only the fit to survive. There always is the danger, however, that the population may not contain individuals with the desired characters; and there is no real justification for the faith that the environment can put them into the plant merely because they are needed.

The initial purity of the plant material which is to be "educated" must be assured before valid conclusions can be drawn regarding the directive effect of environment. Forty years ago it was taught that certain plant disease fungi could adapt themselves to resistant host plants; when grown on a series of increasingly resistant varieties, the fungi were thought to acquire the ability to attack even the most resistant varieties and to transmit this acquired ability to their offspring. The observations on so-called adaptation were sometimes correct, but the explanation was wrong. Species of plant pathogens, like crop plants, may comprise many races that look alike but behave differently and new races are continually being produced by mutation and hybridization. The environment, the resistant host plants in so-called cases of adaptation, did not make the fungus more virulent; it merely eliminated the non-virulent races and permitted the virulent ones to survive.

The extreme diversity of races within a species that has certain very stable structural characters is illustrated by the corn smut fungus. All spores of corn smut, as far as is known, have certain distinct morphologic characters; and yet from two single cells of this organism the writer and his associates have obtained at least 7,500 distinct lines or races that differ in many easily recognizable characters of growth and development, including pathogenicity.

Unfortunately, plant breeders have found from long experience that desire and environment do not father new and better varieties. Certain inexorable laws of genetics cannot be broken, as far as now known, by even the most optimistic and patriotic plant breeder. Heritable factors for good and bad characters may be linked in inheritance. As an example, there is linkage between crown rust resistance in certain varieties of oats and susceptibility to a destructive blight. Naturally breeders would like to combine in a single variety rust resistance and blight resistance. So far, however, this has not been possible. Similarly, some inferior varieties of wheat have high resistance to stem rust but are susceptible to a disease known as black chaff. They have been crossed extensively with rust-susceptible but otherwise superior varieties in order to combine good yielding ability and high quality with rust resistance. Although some good hybrid varieties have been obtained, all of the rust-resistant ones from these crosses are susceptible to black chaff. The linkage between rust resistance and susceptibility to black chaff has not yet been broken. Again, certain vigorous and high-yielding wheats have weak necks, and a high percentage of the heads therefore break off and fall to the ground, especially in hot, dry, windy weather. In attempts to increase the stiffness of the neck by crossing with stiff-neck varieties, the weak-neck varieties have been too stiff-necked to accept the stiff necks. In crosses so far made, hybrids with the desired vigor always have weak necks.

These are only a few examples of the rigidity of the laws of inheritance. Many desirable characters have, of course, been combined, and many crop plants have been greatly improved by hybridization. The fact remains, however, that the good, even if available, sometimes is so closely linked with the bad in inheritance as to create formidable obstacles to improvement. Wishful thinking and authoritarian decrees do not change the laws of genetics.

Progress in plant and animal improvement may sometimes result from lucky chance, but the results often are illusory or transitory. The improvement of plants and animals will be most rapid and most permanent when the work is done with clear recognition of the possibilities and limitations imposed by genetic laws. Dependence on presuppositions and desires simply has not worked. In this type of work, sound science is still a better guide than a teleological philosophy. The safest and surest way to attain practical results is to utilize as fully as possible the facts and principles of the basic sciences.

Authoritarianism is bad enough when it forces scientists to use a single procedure in attempts to attain practical results, because there almost always are better methods for doing better things at certain times and for certain purposes; and these better methods will not be discovered readily unless there is opportunity to try them. Prescription of method may only delay the progress of technology; but the extension of authoritarianism to thought, whether in the natural sciences or the social sciences, menaces the advancement of civilization itself.

The dangers of authoritarianism are real, regardless of time, place, individuals, institutions, and nations.

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VARIETIES OF ECONOMIC LAW, AND THEIR LIMITING FACTORS

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My point of departure will be the impressive case for inductive study in the social sciences, presented last year by Dr. Young.[1] In economics, inductive study is advancing enormously, especially where statistics can be brought to bear; but I will try to show why I see no chance, in this discipline, of going all the way to the kind of inductive study Dr. Young seems to be suggesting. And my main question will be how to take some of the curse of unreality off from the theoretical analyses which economists are bound to go on making. I will offer three positive suggestions, which I hope some theorists might welcome, though many will undoubtedly disagree.

Theorists have a natural urge toward precise and determinate theorems or laws. But the facts of economic life show little consideration for this urge, and remain, to a large extent, perversely and persistently indeterminate.[2] This is the skeleton in the closet of economic theory. What is a proper attitude for a would-be science, forced to deal with such refractory material? One thing economists do is to construct hypothetical simplified "models." These can be used in two ways: as an approach to reality or as an escape from it. My problem is how to promote the first kind of use and set up safeguards against the second.

The escape from reality is proverbially pathological, and there is some temptation to make a study of the pathology of theoretical economics; but that should properly be reserved for an audience of economists.[3] It is not at all clear that there has been too much hypothetical thinking; but I fear students have yielded to the temptation to refine and elaborate such structures beyond what is warranted by their inevitably-imperfect resemblance to reality. Some are continually being seduced by the alluring and deceptive precision of these simplified models, tending to accept their results as representing what "will happen," without always remembering that it happens only in these models, and that they are hypothetical.[4]

Such results need to be corrected and amplified by inductive verification to the fullest extent possible; always remembering that verification involves a deductive step of a neglected and very exacting sort. Before one can tell whether the facts of the real world verify the theory or not, one must give some answer to the question: "What results should this model lead one to expect in the real world, where conditions are less simple?" Imperfect thinking at this crucial step sometimes vitiates the process of inductive verification, the fault being variously divided between the deductive theorist and the inductive verifier. What is possible in this enormously difficult field of verification is severely limited; and this is at least one reason why deductive theorizing will persist.

We do not just observe facts, we put questions to them; and the different kinds of questions we put mark off different kinds of "economic law" which we are seeking. In these terms, three major areas of economic law may be distinguished, having different characteristics. One is concerned with aggregates, and arises from the question: "What governs the levels and fluctuations of total income, production, and employment?" Another is concerned with the behavior of particular prices and wage adjustments, and what determines them. The question of general levels of prices and wages appears to reach into both fields. A

[1] Young, Donald, Limiting factors in the development of the social sciences, *Proc. Amer. Philos. Soc.* **92**: 325–335, 1948.

[2] Throughout this paper, "indeterminate" will mean "not precisely determined by the factors quantitatively taken account of, or available to be so taken account of, by economists in the formulation of economic 'laws' or theorems." Whether economic behavior, or all behavior, is determinate in the light of hypothetical omniscience, I shall not inquire.

[3] For a recent cautious and tentative experiment in psychoanalysing economics from this standpoint, see Walter A. Weisskopf, Psychological aspects of economic thought, *Jour. Polit. Econ.* **57**: 304–314, 1949.

[4] I recognize that my attitude on this general question will seem naïvely unscientific to that school of thought whose criterion of scientific procedure is: theoretical models that yield precisely-determinate results. I suggest that there is a deeper criterion of scientific quality: namely, facing the facts as to the character of the materials one is dealing with.

third field concerns the choices of individuals—how they choose products, select their occupations, and make other decisions of economic import which they make as individuals.

First, as to aggregates. Here the greater part of economists' present effort is concentrated. The strength of inquiry in this field lies in the growing mass and dependability of statistical data, and the growing power of mathematico-statistical devices for analyzing and interpreting them. This has the defects of its qualities, in the shape of a tendency to act as if inductive evidence consisted exclusively of statistics; and a tendency of the mathematico-statistical processing mechanisms to obscure the original data and to get in the way of a constant awareness of the economic reality, an understanding of which is being sought. Thus the defenses against nonsense results are weakened.

It is ironic that the most powerful instrument now at work in this field is a brilliant deductive formula, elaborated by the late Lord Keynes. This formula interprets income as the other side of spending, with spending as the active determining factor, and capital outlays as the most active variable in spending. There is, however, a two-way relationship, and the key to the use of this formula in explaining the behavior of total income rests in the way in which spending responds to changes in income. About the responses of consumer-spending, a good deal is known: when income increases, most of the added income is spent, but not all. One interpretation concentrates on this fact, taking capital outlays for granted. If this were the whole story, then when income increases, the spending normally resulting from that income would increase by a smaller amount, so that the increase, while magnified, would dampen down to equilibrium. One trouble with this picture is that the result depends on small differences in the movements of two large totals. Graphically presented, it depends on the intersection of two lines which are nearly parallel, and one of which must intersect the other from above to bring about the result. In some representations, this difficulty is disguised by exaggerating the angle of intersection.

But there is a deeper difficulty, in that capital outlays also respond to changes in income, but in more complicated ways, dependent among other things on time, previous movements of income and previous relations of capacity to output. Sometimes capital outlays may change less than income does, but at other times they may change more, and enough more so that an increase of income might lead to a greater increase in total spending. And then the movement would not taper off, but be self-reinforcing so long as this condition persists. (In graphic terms, this means reversing the direction in which the lines intersect.) This corresponds to the observed fact that there are times when movements appear to behave in this self-reinforcing fashion, though not without limit. Ultimately, the movement comes to an end, not in a stable equilibrium, but more generally in a condition that breeds a reversal of the movement. Formulas have been produced to deal with this complication; but they have not yet produced results that can be depended on for prediction. When it comes to calculating how long the self-reinforcing condition lasts, and what happens afterward, there are too many governing factors, some of which are unpredictable.

For our present purpose, the thing to remember is the use of a formula depending on the intersection of two lines of not-very-different slope, and one of which is uncertain in its movements. The result is to represent the outcome as determinate, when it actually contains a considerable margin of indeterminateness.

Another difficulty arises from the fact that these responses do not depend on money income alone, but on real income. In real buying power, an income of $5,000 in 1949 is smaller than an income of $3,000 was in 1940—as professors have reason to be aware. The response of spending and saving to changes in income must be interpreted with this in mind. So the problem of prices and wages cannot be escaped. And it cannot be handled solely by studying the relations of statistical aggregates. They will not tell us why some prices change little while others change much. And they will not tell us whether a wage-increase is going to force up the price of steel and bituminous coal, and thus raise the costs of things in general. To understand the course of inflation and deflation, one must reckon with the forces that determine particular prices and wage-adjustments.

And these depend, in our current economy, on how and why business men and labor leaders make the decisions they do. They work within limits set by objective conditions; but to know where the result must fall, within these limits, it would be necessary to get inside the minds and counsels of the organizations that make the decisions.[5]

[5] The objective record tells what happened to prices and output, but not why. Schools of business have an entree to collect case-material; but so far, this material has not

And impartial investigators from the social sciences are not often invited into these counsels, for reasons that are very compelling. The labor leader is engaged in a diplomatic poker-game on behalf of a politically-organized group, and he is not showing his hand. The business man operates with one eye on his competitors and the other on the anti-trust laws. Some pertinent facts he keeps from his competitors; and he has learned by sad experience that practices that seem utterly justifiable to him may look different to the anti-trust authorities. Such testimony as he volunteers, though it may be sincere, is seldom to be fully counted on to take in all the important factors.

There are some rifts in this iron curtain and there may be more in future, but really enlightening ones are still exceptional, and likely to remain limited in number and extent. Hall and Hitch, in their Oxford study, secured answers from thirty-eight business men as to how they make their prices, the answers pointing to a prevalent pattern of pricing to cover cost plus a "reasonable" profit.[6] Professor Edmund P. Learned of the Harvard Business School has recently made a more intensive study of the pricing of gasoline by the Standard Oil Company of Ohio, with the company's full cooperation.[7] This result and that of the Oxford study may be more nearly reconcilable than appears on the surface, especially if more were known about the competitive conditions that limit the English businesses. Both reveal margins of discretion, and the Ohio study sheds light on the somewhat elastic limits within which it acts.

One thing that is known is that the patterns in different industries are different; and another thing that seems fairly certain is that these patterns change over time. We should not look for one law of price applicable to all industries; and it is not safe to assume that, if we find the law that operates in one industry, it will remain unchanged indefinitely. This creates another difficulty for inductive study. If investigators could pierce the barriers in one industry after another, they might hope ultimately to cover the whole field; but long before this happened, their earlier studies would be dated, and would need to be done over again. They could never hope to catch up with changing conditions and evolving practices. The yearning of the student for a fixed and determinate law is in conflict with the American spirit of experiment and innovation, epitomized in the aphorism attributed to Gantt: "Show me how you're doing it and I'll show you one way that is wrong." The characteristic American reaction to the invention of a better mousetrap is to look for another that will be better yet. This spirit operates in the realm of trade practices as well as in that of technical processes, and it has no end.

It may be rerouted—not terminated—when someone with authority or power sets up rules, to which practices must conform. And then something happens to the traditional relation between economic law and politics, in the broadest sense of power-decrees, official or unofficial. There was a time, following the period of faith in an overruling "law of nature" which Professor Viner described so well last spring, when "economic law" purported to determine what must inevitably happen, and why political attempts to interfere were futile or worse. Nowadays, what must happen is determined by the Agricultural Administration, or the Federal Trade Commission, or by an agreement between John Lewis and the coal operators, with or without the intervention of a fact-finding board. And then the question arises what role, if any, is left for "economic law" to play. One of my three suggestions will bear on this question.

Finally, we come to the third major field of inquiry, how people make their individual economic choices. Here there is great indeterminateness, plus the still more basic difficulty that we are dealing with a wide variety of qualitatively-different human values, for which there is no objective common measure, or none that is not open to objections. Economists have commonly escaped from both these difficulties by falling back on the purely formal idea that everybody has some order of preference, and follows it. This is held to be true, whatever his wants may be, and whether they make for welfare or the opposite—hence there is no need to raise these questions.

But this comes perilously close to being an escape into sterile tautology, equally consistent with any possible behavior and therefore telling us nothing, and impossible to prove or disprove by evidence. It grows out of a commendable desire to be scientifically neutral between values, and not to set up one's personal judgment as to what is the greater value and what is the less. But it is not the answer.

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illuminated and transformed economists' views of their central problems.

[6] Hall and Hitch, Price theory and business behavior, *Oxford Econ. Papers* No. 2: 12–45, 1939.

[7] Pricing of gasoline: a case study, *Harvard Business Rev.* **26**: 723–756, 1948.

Psychology can tell us more. It does not offer formulas from which we can deduce universal laws in this field, but we should not expect that. It does not do away with the need of studying how people behave in actual economic situations: in fact, it points to the necessity of such study. And it furnishes useful suggestions of things to look for—something a tautological universal cannot do. But there will always be novel situations, in which even mass behavior is unpredictable, despite the benefit of large numbers—for example, the behavior of American consumers in the first three postwar years.

So much for the difficulties of the study of economics. If my picture is correct, it is a field in which neither inductive nor deductive study can hope to give precise and definitive answers to all pressingly-important problems. Facing this dilemma, I wish in closing to make three constructive suggestions for meeting it, mainly in the theoretical sector of study.

First, as to our dealings with ultimate human values. Since we are inescapably committed to dealing with them, and anything we do about them has implications, I suggest that we grasp this nettle boldly and frankly. Instead of attaching great importance to a formal analysis of choice in the abstract, devoid of content, I suggest that we deal with the content of values—for example, health—and tell what the economic system does to it; or what kinds of values the system inherently tends to promote or to neglect. This often carries its own verdict automatically, on the basis of existing and dominant judgments of values. An economist need not fear he is airing personal tastes and arbitrary prejudices if he assumes that the American people want to be healthy, or if he reports that the unregulated market permits harmful preservatives to be used in foods.

It is time we abandoned the idea that an individual can always choose for himself better than anyone else can choose for him. If my doctor does not know better than I do about the needs of my internal workings, then medical science is a failure. And economists need not assume that they alone, of all people in the society, are ignorant of that fact; they, like other social students, know where superior knowledge is to be found. Even aside from this, a well-founded popular judgment, based on a comprehensive picture of the facts, often has more claims to validity than the choices of uninstructed individuals; and the individuals' afterthoughts may have more claim to validity than their choices at the moment of purchase or other decision.[8] An economist has no need to apologize for relating his picture of the facts to such standards, instead of solely to the verdict of the market-place, which is a biased verdict, for reasons that can easily be formulated. The only requirement for objectivity is that, when an economist relates the facts to standards other than those of the market, he should be explicit as to what these standards are. This, and not enslavement to market standards, is the genuinely objective way of conducting our inevitable dealings with human values.

The second suggestion relates to the kind of theoretical analysis, generally of conditions of equilibrium, commonly expressed by lines in graphs, centering on maxima, minima, and points of intersection. The suggestion is that for lines we substitute bands or zones with a width gauged to represent the indeterminateness which the hard lines conceal.[9] This would turn a maximum or an "optimum" into a range, not a point, and would make it incumbent on the student to do something about the question how wide this range is likely to be. If the curve in question is fairly flat, vertically, the horizontal range of indeterminateness may be very wide—as is the case, for example, with many curves of the relation of cost to size of plant. The evidence seldom reveals any one definite size as most efficient.

This would force attention to a fresh group of problems: namely, within this range in which results are indeterminate by the factors entering into the curve, what other factors determine the actual result, and how do they act? This change might even be refreshing to some theorists, who may be

[8] I suspect that Justice Holmes came nearer than the designers of indifference curves to defining an ultimate standard of human valuation.

[9] F. W. Taussig's "penumbra of supply and demand" was a pioneering use of this concept. *Cf.* also J. R. Hicks, The theory of wages, 61, N. Y., P. Smith, 1948, where a "range of indeterminateness" is included in the analysis. My first use of this device was in *Economics of planning public works*, 95, 99, Nat. Planning Board, 1935, and was concerned with the "multiplier effect" of continued public works expenditures. I note that it has been used more recently by Prof. Wilford J. Eiteman, *Price determination: business practice versus economic theory*, 25–27, 32–33, 38, Ann Arbor, Univ. of Mich., 1949.

For some purposes, it might be appropriate to start with a point representing initial values of the variables, with the zone of indeterminateness fanning out in both directions. As applied to human choices, the zone technique might rescue the element of validity in marginal utility theory, by separating the zone within which preference is undoubted, from the zone of uncertainty and indeterminateness.

getting just a trifle tired of elaborating deductive structures dependent on the fictitious precision of these curves, and going beyond what is warranted by the rough and approximate correspondence of reality to the premises on which the models are built.

One of the neglected variables is time. The orthodox demand-curve, for example—probably the most familiar curve in economics—is a timeless schedule of different volumes of sales that can be made at different prices. Convert this into a process of adjustment to a change in price, the process requiring time, and a vast field is opened up, now occupied in a fragmentary and unsystematic way. Students of particular problems sometimes recognize this time-factor. Theory already takes account of expectations of future changes in price as a modifying factor, and there is an embryonic theory of overcorrection and resulting oscillation.[10] As to duration of the adjustment process, some adjustments—say on produce markets—are virtually instantaneous; while others—say the adjustment of ways of living to changes in the relative cost of domestic servants and mechanical household equipment—may require generations and ramify much farther than a mere price-quantity adjustment. There are changes that may approach a static asymptote, and others for which the only appropriate assumption would be indefinite progressive growth. For changes of this last sort, the orthodox type of static schedule is basically misleading.

My third and last suggestion concerns what can happen to "economic law" when its field is preempted by power-enforced decisions. In the days of the simple competitive theory, the same model used to serve two functions: as a first approximation to reality and as a standard of sound practice. As an approximation to reality, it has been badly damaged; but it has lately been reconditioned to describe how a socialist state *might* organize its system soundly, if it were intelligent enough to take the economists' advice. My suggestion is merely that the rest of us take the hint, and recognize that there is a place for a theory of sound practice that does not purport to describe the actual system. To expect the same theory to do both jobs is one way of assuming Utopia. Ideally, an economist should be able to approve of something as sound without being understood as predicting that it will come to pass; or he should be able to predict something without being construed as advocating it. It is obvious that sound standards may be violated, not only by political power-decisions, but also by the workings of the market itself—for example, when it precipitates us into a depression.

A theory of sound practice is, of course, necessarily deductive, and can be carried out with various degrees of hypothetical freedom from the power forces, or various degrees of recognition of the necessity of making some concessions to them. But it should accept the inevitable conditions of industry. To make it harder for those who tend to retreat from reality, one might stipulate that the aim be something that could conceivably throw light on some actual decision: for example, on the question what would be an economically correct wage for producers of steel, automobiles, or bituminous coal. Or, instead of refining formal utility curves or indifference curves, devoid of content, the question would be: "What should a wise government do about consumers' freedom of choice? What things would it leave to be so decided by individual consumers, and under what guidance or other influence? What things would be otherwise decided, how and why?" Needless to say, the results of such an inquiry would not be final. But they should be rich in hypotheses fit for testing, and in problems which the current abstract theoretical treatment slides over. And merely putting the question in the form I have suggested would dictate certain general features of the answer, beyond reasonable doubt. I believe there is enough we can be sure of, to expose and tend to discredit various irrelevancies and downright errors implicit in accepted theorizing on this question, and to do it by offering a positive substitute, not by merely negative criticism, which is always ineffective if no positive substitute is offered.[11]

These suggestions face the stubborn fact that we deal with indeterminate material and with

[10] The most definite model, the ingenious "cobweb theorem," explores the valid and important concept of adjustment proceeding by discontinuous steps, like the seasonal moves in agriculture. But it has reached conclusions which can, I believe, be shown to be at best ambiguous and at worst false, owing to a mechanical and unrealistic handling of the extremely tricky concept of a supply schedule. It is an excellent example of the danger of drawing formal conclusions, merely from lines on charts.

[11] I have made this attempt, very briefly and simply, in *Guideposts in time of change*, 66–72, esp. p. 68, footnote, N. Y., Harper, 1949.

incommensurable human values. If adopted, they would open up a deal of work for theorists, and offer mechanisms by which their theories might be made more realistic and objective, and at once more quantitative and less precise. If successful, such efforts should enrich inductive research, by furnishing hypotheses which it might usefully verify, in place of tautologies or exercises in algebraic or geometrical ingenuity, carried beyond the limits of realism. And not least, they should make for a closer and more constructive working connection between inductive study, which affords the ultimate test of verification, and theoretical analysis, which we can never do without.

JOSEPH PRIESTLEY AND THE AMERICAN PHILOSOPHICAL SOCIETY: HIS EXPERIMENTS ON SPONTANEOUS GENERATION

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(*Read November 5, 1949*)

I. THE CONNECTION OF JOSEPH PRIESTLEY WITH THE AMERICAN PHILOSOPHICAL SOCIETY

IT IS generally known that Joseph Priestley was a distinguished philosopher, the discoverer of oxygen, and a founder of the science of chemistry, but his relations with Benjamin Franklin and the American Philosophical Society are not so widely known. He was born in 1733 and was therefore twenty-seven years younger than Franklin and ten years older than Franklin's American Philosophical Society. During Franklin's long residence in London (1757–1762, 1764–1775), he became intimately acquainted with young Priestley and they were associated in many scientific, political, and social matters.

Franklin was a sponsor of Priestley when he was elected a Fellow of the Royal Society in 1766, as well as in his election to membership in the French Academy of Sciences in 1772. But for some unknown reason Priestley was not elected a foreign member of the American Philosophical Society until 1785, although his great French co-discoverer of oxygen, Lavoisier, was elected ten years earlier.

Priestley, with Edmund Burke and many other loyal friends of Franklin, was present at his arraignment before the Privy Council, January 11, 1774, when for nearly an hour he stood and endured the denunciation and abuse of the Solicitor General without uttering a word. The next morning at breakfast Franklin told Priestley that he considered this one of the best actions of his life and that he would do it again in the same circumstances. His last day in London, March 19, 1775, he spent with Priestley from morning to night in sad converse over the coming war which he had labored so long and so hard to prevent.

According to the minutes of the American Philosophical Society, Priestley's connection with the Society began when in 1772 he presented his Treatise on Optics to the Society. In 1784 Samuel Vaughan presented a manuscript copy of "Priestley's experiments on Phlogiston and the seeming concoction of water into air"; in the same year John Vaughan, Librarian of the Society, presented thirty-three volumes of his works to the Library, many of them doubtless on theology, for the Rev. Joseph Priestley was a dissenting clergyman and leader in the Unitarian denomination. Vaughan and Priestley were co-religionists in that they held similar beliefs regarding Unitarianism and when Priestley came to America both were active in promoting that faith here. As previously stated, Priestley was elected a foreign member of the American Philosophical Society in 1785, and his certificate of membership is now in the archives of this Society. In 1791 a "profile of him in Plaster of Paris, particularly valuable for its resemblance" was presented by Vaughan, and a beautiful Wedgewood bas-relief of him is now on exhibition in the Hall.

Priestley was a liberal, possibly a radical for those times, in politics and religion, and a great admirer of the revolutionary movements in France and America. On the second anniversary of Bastille Day, July 14, 1791, a mob in Birmingham, England, where he lived, stormed his house shouting "Down with Philosophers! Church and King forever!" They destroyed all his philosophical apparatus, his books and manuscripts, the results of years of work, and then burned his house to the ground. Priestley and his family escaped with their lives, fled to London, and never returned to Birmingham.

During his life in Birmingham Priestley had been a member of a remarkable club of philosophers and inventors, not unlike Franklin's Junto, and known facetiously as the "Lunar Society," its members as "Lunatics." Dr. Erasmus Darwin, grandfather of Charles Darwin, was President of this society, which Hesketh Pearson, in his book *Doctor Darwin,* says was "the most remarkable group of thinkers and inventors of the 18th century." Priestley was highly respected and deeply

loved by members of this society and they tried to save him and his property from the mob, and afterwards to make it possible for him to return to Birmingham, but all to no avail. Thus, cruelly uprooted in his native land, in his sixty-first year he emigrated to America.

He and some members of his family landed in New York on June 5 and came on to Philadelphia on June 20, 1794. A Committee of the American Philosophical Society, consisting of President Rittenhouse and five other members, prepared a Congratulatory Address to Dr. Priestley which was presented to him at a special meeting of the Society on the following day. This address and Priestley's response which are published in full in the Early Minutes of the Society [1] are too long to quote here but a few sentences may indicate their general character:

> The American Philosophical Society . . . [reads the Address] offer you their sincere congratulations on your safe arrival in this country . . . the Society felicitate themselves and their country that your talents and virtues have been transferred to this Republic. . . . In this free and happy country those unalienable rights which the Author of Nature committed to man as a sacred deposite have been secured. Here we have been enabled, under the favor of Divine Providence, to establish a government of laws and not of men; a government which secures to its citizens equal rights and equal liberty; and which offers an asylum to the good, to the persecuted, and to the oppressed of other climes. May you long enjoy every blessing which an elevated and highly cultivated mind, a pure conscience, and a free country are capable of bestowing.

To this address Priestley replied in part:

> It is with peculiar satisfaction that I receive the congratulations of my brethren of the Philosophical Society in this city, on my arrival in this country. It is, in great part, for the sake of pursuing our common studies without molestation, tho' for the present you will allow, with far less advantage, that I left my native country, and have come to America; and a Society of philosophers, who will have no objection to a person on account of his political or religious sentiments, will be as grateful as it will be new to me . . . you may depend upon my *zeal* in promoting the valuable objects of your institution, but you must not flatter yourselves, or me, . . . that, at any time of life, . . . I can be of much service to it. I am confident . . . that it will soon appear that republican governments . . . will be far more favourable to Science and the arts, than any monarchical government has ever been. The patronage to be met with there, is ever capricious . . . having for its real object not science, nor anything useful to mankind, but the mere reputation of the patron, who is seldom any judge of science. Whereas a *Republic*, which neither flatters, nor is to be flattered, will not fail in due time to distinguish true merit. . . .

It is well known that Dr. Priestley declined an invitation to remain in Philadelphia at the University of Pennsylvania and among his brethren of the American Philosophical Society, and that he proceeded, with certain "friends of liberty" in government, learning, and religion, to establish a settlement in Northumberland County, Pennsylvania. Details regarding this proposed settlement have recently been explored and published by Mary Cathryne Park in a Ph.D. dissertation in the Department of English at the University of Pennsylvania under the title "Joseph Priestley and the Problem of Pantisocracy." [2]

Dr. Park has traced in admirable detail the connection of Joseph Priestley with a grand project to establish in America a settlement for "Friends of Liberty," or a "Pantisocracy," the term proposed by Robert Southey and Samuel Taylor Coleridge, in which all should be equally secure in the development of civic, philosophic, and religious liberty. This ideal was especially dear to Dr. Priestley and to many other persons in England, including particularly the poets Southey and Coleridge. It is the connection of these distinguished literary men with this proposed settlement that occupies an important part of this publication by Dr. Park and gives significance to its presentation as a dissertation in the Department of English of the University of Pennsylvania.

Dr. Priestley entrusted to his son, Joseph Priestley, Jr., and to his son-in-law, Thomas Cooper, the selection of the site for the settlement, and Dr. Park's search of the records in the Land Office in Harrisburg and in the Ridgeway Library in Philadelphia has revealed that Joseph Priestley Jr., John Vaughan *et al.* had purchased before Dr. Priestley arrived in Northumberland approximately 120 tracts of about 400 acres each, and 20 tracts of about 1,000 acres each, and after Dr. Priestley's arrival and before the end of 1796, 35 tracts of about 400 acres each, and 88 tracts of about 1,000 acres each, or a total of 263 tracts of approximately 170,000 acres, while 6 other tracts of much greater area, which were contracted for by other cooperators in this land settlement,

[1] *Proc. Amer. Philos. Soc.* **22** (3): 223–224, 1884.

[2] Univ. of Penna., Reprint from *Proc. Del. Co. Inst. of Science* **11** (1), 1947.

added 544,000 acres. After selection of the site, Thomas Cooper returned to England to publish his prospectus of the settlement entitled *Some Information Respecting America*. But in spite of the glowing account of this enterprise and the support of the pantisocratists, Southey and Coleridge, as well as others, the settlement failed, like so many other Utopias.

Dr. Priestley continued to reside in Northumberland and to carry on his researches there until his death in 1804. In the last year of his life the Society on February 28, 1803 resolved "to dine together on Saturday next (March 5) . . . and to request Dr. Priestley's company, informing him that the Society are induced to make this request from their high respect for his Philosophical Labors and discoveries and to enjoy the more particular pleasure of a social meeting." Priestley's acceptance was read to the Society at a meeting on March 4 and the dinner occurred the following day.

On the announcement to the Society of his death on February 6, 1804, Benjamin Smith Barton was elected to deliver a Eulogium, and on June 3, 1805 the members marched in procession from the Hall to the First Presbyterian Church on Market Street where the Eulogium was pronounced before a large audience. Finally on April 3, 1818 Mrs. Caspar Wistar, widow of President Wistar, presented to the Society the portrait of Priestley by Rembrandt Peale "long in possession of our late president," which now hangs in our Hall.

During the last ten years of his life Priestley sent to the Society to be read and published in the *Transactions* ten different papers on his experiments and observations relating to (1) The analysis of atmospherical air, (2) The generation of air from water, (3) Transmission of acids, and other liquors, in the form of vapour, over several substances in a hot earthen tube, (4) The change of place in different kinds of air through several interposing substances, (5) The absorption of air by water, (6) Miscellaneous experiments relating to the doctrine of Phlogiston, (7) On the production of air by the freezing of water, (8) On air exposed to heat in metallic tubes, (10) A letter to Dr. Wistar on the discovery of nitre in common salt which had been frequently mixed with snow, and (9) the last important paper of his life, "Observations and Experiments relating to equivocal, or spontaneous, Generation." All of these papers were published in the *Transactions* of the American Philosophical Society, old series, volumes 4–6, 1799–1804.

II. A GENERALLY OVERLOOKED PAPER BY JOSEPH PRIESTLEY ON SPONTANEOUS GENERATION

This paper by Dr. Priestley is of such interest in the long controversy regarding spontaneous generation that it seems to me to deserve to be more widely known by persons interested in the history of science. Since I have not been able to find any reference to it in the principal works on this subject, not even in Pasteur's magnificent monographs, and since the volume of the *Transactions* in which it appears is rare and no longer available, a brief summary of the general state of scientific opinion regarding spontaneous generation at the end of the eighteenth century, and an account of Priestley's experiments are presented here. Dr. Priestley begins his paper as follows:[3]

> There is nothing in modern philosophy that appears to me so extraordinary, as the revival of what has long been considered as the exploded doctrine of *equivocal*, or, as Dr. Darwin calls it, *spontaneous generation;* by which is meant the production of organized bodies from substances that have no organization, as plants and animals from no pre-existing germs of the same kinds, plants without seeds, and animals without sexual intercourse.

He follows this introductory sentence by the statement that in the seed or embrio [*sic*] there is present the future plant or animal in miniature, as was maintained by the preformationists of that era and follows this with the statement that the doctrine that "the smallest insect as well as . . . the largest animal arise without the interposition of a creative power from . . . *brute matter*—earth, water, or mucilage," or even organic particles, without any intelligent cause, has been completely exploded. His maintenance of the doctrine that the animal or plant is preformed in all its parts in the germ from which it comes, and that species arise only by special creation, is as antiquated as his Phlogiston theory, but his denunciation of the doctrine that either animals or plants arise from his "brute matter" is justified by modern research. His mistake was in supposing that the germs from which all plants and animals arise are miniatures of the mature forms into which they develop, but his experiments by which he proved that a living water plant is not produced by the water in which it appears but from spores or seeds is thoroughly modern.

The immediate occasion for this venture by Priestley into a field which he had never entered before, was the publication of a book by his old

[3] *Trans. Amer. Philos. Soc.* (o.s.) **6**: 119, 1804.

friend, Dr. Erasmus Darwin, entitled *The Temple of Nature*, a poem of 136 pages and many additional pages of notes, which was published in England in 1802, and in New York in 1804.

In the body of the poem Darwin affirms the origin of life from the non-living in the following, and many other, lines:

> Hence without parents by spontaneous birth
> Rise the first specks of animated earth.[4]

Darwin's note on these lines, *Temple of Nature*,[5] reads:

> Not only microscopic animals appear to be produced by a spontaneous vital process, and then quickly improve by solitary generation, like the buds of trees, or like the polypus and aphis; but there is one vegetable body, which appears to be produced by a spontaneous vital process, and is believed to be propagated and enlarged in so short a time by solitary generation as to become visible to the naked eye; I mean the green matter first attended to by Dr. Priestley,[6] and called by him conferva fontinalis. The proofs that this material is a vegetable, are from its giving up so much oxygen, when exposed to the sunshine, as it grows in water, and from its green colour.
>
> Dr. Ingenhautz asserts, that by filling a bottle with well-water, and inverting it immediately into a basin of well-water, this green vegetable is formed in great quantity; and he believes that the water itself, or some substance contained in the water, is converted into this kind of vegetation, which then quickly propagates itself.
>
> Mr. Girtanner asserts that this green vegetable matter is not produced by water and heat alone, but requires the sun's light for this purpose, as he observed by many experiments, and thinks it arises from decomposing water deprived of a part of its oxygen, and laughs at Dr. Priestley for believing that the seeds of this conferva, and the parents of microscopic animals, exist universally in the atmosphere, and penetrate the sides of glass jars. Phil. Mag. for May, 1800.

In response to this Dr. Priestley wrote: "That various animalcules as well as seeds float in the atmosphere is unquestionable, but that they pass through glass I never heard before."

Dr. Darwin says with reference to this work of Priestley:

> The green vegetable matter of Dr. Priestley's which is universally produced in stagnant water, and the mucor, or mouldiness, which is seen on the surface of all putrid vegetable and animal matter, have probably no parents, but a spontaneous origin from the congress of the decomposing organic particles, and afterwards propagate themselves. [p. 147.]

To this Priestley replies:

> Let us now compare *this* language with that of *nature* in my experiments. On the first of July [1803] I placed in the open air several vessels containing pump-water, two of them covered with olive oil, one in a phial with a ground glass stopper, one with a loose tin cover, and the rest with the surface of the water exposed to the atmosphere; and having found (as may be seen in the account of my former experiments on this green matter) that it was produced with the greatest facility, and in the greatest abundance, when a small quantity of vegetable matter, especially thin slices of raw potatoes, was put into the water, I put equal quantities, viz. twenty grains of potatoe, into each of the larger vessels and ten into each of the smaller. Into two very large decanters, the mouths of which were narrow, I put fifty grains of the same, one of them having oil on its surface, and the other none. At the same time having filled a large phial with the same water, I inverted it in a vessel of mercury.
>
> In about a week the wide mouthed open vessel began to have green matter, and the large decanter with the narrow mouth had the same appearance in three weeks. On the first of August the vessel which had a loose tin cover, coming about half an inch below its edge, had a slight tinge of green, and on the first of September the phial with the ground glass stopper (but which, appeared by some of the water escaping, not to fit exactly) began to have green matter. But none of the vessels that were covered with oil, or that which had its mouth inverted in mercury, had any green matter at all on the 12th of September; when, having waited as I thought long enough, I put an end to the experiment.
>
> Here we see that the wider was the mouth of the vessel, the sooner did the green matter appear in it; but that in time the germ (or whatever it may be called that produced it) found its way through the smallest apertures . . . but that when all access to the water was precluded by a covering of oil, or a quantity of mercury no green matter was produced. [p. 124.]

Further criticisms of Dr. Darwin's supposed *facts* of spontaneous generation are given, after which Priestley considers, "all that Dr. Darwin had advanced by way of *argument* in defense of the system that he has espoused." First as to the supposed analogy between nutrition and spontane-

[4] *The Temple of Nature*, Canto I, l. 247, N. Y., 1804.

[5] *Ibid.*, additional notes, pp. 141–142.

[6] This reference to work of Priestley on green vegetable matter is to his *Experiments and Observations on different kinds of Air*, 3 (1), sec. 8, entitled "Further Observations on the green vegetable matter with which many of the preceding experiments were made," Birmingham, 1790.

ous generation, i.e., dead organic matter contributing to the nourishment of plants and animals just as "the same particles of organic matter may form microscopic animals, or microscopic vegetables, by chemical dissolutions and new combinations of organic matter." Of this argument Priestley says:

> But these microscopic vegetables and animals, there is every reason to think, have as complete and exquisite an organic structure as the larger plants and animals, and have as evident marks of design in their organization, and therefore could not have been formed by any decomposition or composition of such dead matter, whether called *organic* or not, without the interposition of an intelligent author. Besides these microscopic vegetables and animals are infinitely various, and therefore could never arise from the same dead materials, in the same circumstances, by the mere application of warmth and moisture. Each of these vegetables and animals must, according to the analogy of nature, have proceeded from an organized germ, containing all the necessary parts of the future plant or animal. . . . [p. 125.]
>
> Microscopic vegetables and animals remaining without any visible sign of life months and years is no proof that they were capable of deriving their origin from dead unorganized matter. While their organization is not destroyed the motions which indicate life may be restored by proper degrees of heat and moisture; but this is not materially different from the case of frogs and other animals, which discover no sign of life, a great part of the winter, and revive with the warmth of spring. [p. 126.]

In reply to Dr. Darwin's argument that

> . . . these microscopic organic bodies are multiplied and enlarged by solitary reproduction, without sexual intercourse till they acquire greater perfection or new properties. Lewenhoek [*sic*] observed in rain-water which had stood a few days, the smallest scarcely visible microscopic animalcules, and in a few more days he observed others eight times as large. English Encyclop. Art. Animalcule. [p. 146.]

Priestley replies:

> But this proves nothing more than an increase in bulk, and no change of a small animal into a larger of a different kind, which the argument requires. . . . That several insects are multiplied without sexual intercourse is no proof of spontaneous generation. . . . [p. 126–127.]
>
> Dr. Darwin, and all other advocates for spontaneous generation, speaks of some animals as *simple* and others as *complete*, some as *imperfect* and others as *perfect;* whereas, as far as we can discover, all animals, even the most minute that have been examined, appear to be as perfect, and to have a structure as wonderfully complicated, as the largest, though on account of their minuteness, we cannot dissect them to so much advantage. Their organs are equally adapted to their situations and occasions; and what is more, they have as great a degree of *intelligence* (which they discover by the methods of seeking their food, avoiding, or contending with their enemies) as the largest animals: besides, it is never pretended that any large species of animals, though called imperfect, as crabs and oysters, &c., are ever produced by spontaneous generation.

After quoting from Dr. Darwin several other cases of supposed spontaneous generation among fungi, animalcules, and even different kinds of worms Priestley says:

> These are only random observations, and the facts have never been investigated in an accurate philosophical manner. . . . It is, besides, unworthy of philosophers to draw important conclusions from mere ignorance. [p. 124.]

Finally Priestley presents arguments against the transmutation of species as well as spontaneous generation, both of which he thinks involve *atheism*. These conclusions are interesting as showing the opinion of a notable observer and experimenter at the beginning of the nineteenth century. But his actual experiments proving that spontaneout generation does not occur in the case of *Conferva fontinalis* deserve to be better known.

DAY BOOK OF AN EDUCATION: WILLIAM SHIPPEN'S STUDENT DAYS IN LONDON (1759–1760) AND HIS SUBSEQUENT CAREER

BETSY COPPING CORNER

(*Read November 4, 1949*)

LATE one November afternoon in 1762 a distinguished audience gathered at the State House in Philadelphia, now Independence Hall, to hear a young man of twenty-six deliver a lecture. We do not know exactly what was said; we do not know exactly who was there. How then am I justified in saying a distinguished audience gathered and why was this an event important enough to be recalled?

Dr. William Shippen, Jr., lately returned from four years of medical study in England and Scotland, was the lecturer. The occasion for which the audience assembled was the delivery of his Introductory Lecture to a course in anatomy which marks the institution of formal medical instruction in the American colonies. Apparently the lecture was never printed and whether the original manuscript survives is a question to which we can hardly expect a favorable answer. If contained among the Shippen Family Papers it is not available at present for inspection. It is certain that when this lecture was originally delivered on the evening of November 16, 1762, it attracted attention from all members of the medical profession in Philadelphia, from the managers of the Pennsylvania Hospital just completing its first decade of public service, from the relatives and friends of young Dr. Shippen and from the small group of young students about to begin attendance at his course of lectures and demonstrations which was to follow almost immediately in quarters especially adapted for the purpose at his father's residence in Fourth Street.

The physicians especially interested in this performance were in addition to the young man's father, Dr. William Shippen, the elder, a group of Philadelphia's well-known doctors to whom young Shippen had dedicated his thesis, *De Placentae cum Utero Nexu,* only the year before, when he received his M.D. degree in 1761 from the University of Edinburgh.

In the resounding Latin of his dedication he had described these associates of his father in the medical profession, the well-known Philadelphia doctors—Thomas Bond, Phineas Bond, Thomas Cadwallader, John Redman, and Cadwallader Evans—as *Viris Spectatissimis,* the most outstanding men. *Medicis peritissimis,* he continues, *ob morum humanitatem et in miseros clementiam, valde conspicuis et imitatione dignis, quorum societatem et amicitiam, ex animo cupit et colit.*

As for the rest of the audience, in addition to these distinguished medical figures in the city, was Benjamin Franklin there? Who can tell? He was in Philadelphia that autumn, having arrived on November 1, only two weeks before Shippen's lecture, after absence in England for five years largely devoted to provincial business. Franklin had been friendly to young Shippen when he met him in London in 1759–1760. They had dined together more than once, Franklin had given him the opportunity of spending an evening at the Royal Society, and later on Twelfth Night they had gone to Court together to see the Royal Family. On this occasion they had been out until two in the morning and Shippen had lost his hat "in the midst of the greatest and best mob I ever saw" he reports in his diary. More than a year later, after Shippen's doctoral thesis had been completed, the newly created doctor of medicine sent Franklin a copy as soon as it was printed in September 1761. What more likely, after this degree of intimacy, than that Franklin would graciously contribute the prestige of his presence at his young protege's first professional lecture at the start of his lifework in Philadelphia? Certainly upon this occasion representatives of the College of Philadelphia would be present from professional interest in an educational enterprise undertaken by the son of one of their prominent trustees. Dr. William Smith, Provost of the College, was very likely in the audience. He, too, had taken a liking to the young lecturer whom he had seen during the summer of 1759 in London where they had lunched together shortly before Dr. Smith, having straightened out his tangled affairs, embarked for Philadelphia. At that time William Shippen, Jr., had entrusted to Dr. Smith's care some precious letters

to his family in Philadelphia, and now Shippen would be looking for the blessing of the eminent Dr. Smith's presence at the Introductory Lecture on anatomy. November 16, 1762 marks the start of William Shippen's long professional career as an anatomist. Three years later in 1765, he broadened his scope—having overcome popular prejudice against dissection of the human body—by instituting lectures in midwifery, the first obstetrical instruction of any kind ever offered in this country and open to both sexes. At the same time, in Laetitia Court near the waterfront, Shippen set up a small hospital for the lying-in care of poor women and the instruction of students in obstetrical procedure. That same year of 1765 when the College of Philadelphia instituted its medical department, largely through the organizing powers of John Morgan who became Professor of the Theory and Practice of Medicine, William Shippen, Jr., received the second appointment as Professor of Anatomy and Surgery. The circumstances preceding these appointments, which elevated Morgan in the public eye and reduced Shippen to secondary standing in an undertaking for which he had long been quietly at work, were galling to Shippen, but he bore the situation with outward calm. His subsequent career as professor and practitioner, broken only by a period of service not altogether fortunate as director general of the military hospitals of the American Army during the Revolution, has been authoritatively recorded by your fellow member, Dr. Francis R. Packard. From lack of documentary material no full length biography of William Shippen, Jr., has ever been attempted. He was no writer himself, not from lack of ability, but because of his absorption in his mission as medical educator and his delight in social relaxations. The only contributions from his pen that we know of during his period of professional activity are one letter he wrote in 1783 to William Cullen of the medical faculty of the University of Edinburgh and a Commencement address or charge to the medical students which Shippen delivered at graduation exercises at the University of Pennsylvania in 1790.

No details have been available hitherto about Dr. William Shippen, Jr.'s medical training until a little diary kept by him while a student in London was discovered some years ago in a most unlikely manner and place by Dr. J. Hall Pleasants of Baltimore. The story of this find is as follows. Books and papers belonging originally to Dr. William Shippen, Jr., had come through inheritance into the possession of his kinsman, Dr. Charles Shippen, a nineteenth-century physician who lived and practised in Baltimore. When Dr. Charles Shippen died, disposition of his medical books and papers became a problem and his heirs called upon their friend, Dr. Pleasants, for advice. In the midst of this task and when the classification of the books was almost finished, Dr. Pleasants happened to detect on the attic floor a booklet in manuscript. Sight of the handwriting roused his curiosity and upon examination he found that this was a diary, a forty-page daily record kept by William Shippen, Jr., while he was studying medicine in London. The record runs from July 19, 1759 to January 22, 1760. This little diary came into my hands for study through the courtesy and generosity of Dr. Pleasants who brought it to our house one day last autumn because of his interest in the Autobiography of Benjamin Rush, lately edited by Dr. Corner.

At first glance the diary is disappointing. Its entries are brief, a mere hurried notation of what each day has brought in work, pleasure and association with other people. As one re-reads its pages, however, its suggestive power increases. It is as if a text had been supplied without a sermon. The reader must make the application of its revelations for himself. A fact is stated, he must dig for the meaning. As I have worked out the eighteenth-century allusions to people, places and events I have come to regard the diary almost as a crossword puzzle in reverse—the *words* are given there in the daily entries; the *definitions* must be laboriously sought.

The diary young Dr. Shippen kept in London that eventful year of 1759, the Annus Mirabilis of English history when the British Empire was rapidly taking shape, is a youthful document revealing a succession of enthusiasms. In its pages we see Shippen under the sway of four important men, four men representing in their turn the appeal of the church, the stage, and the dissecting room. First comes George Whitefield, the roving eighteenth-century evangelist, a family friend and curiously enough a fellow alumnus of the College of New Jersey who was holding British audiences enthralled through the oratorical power of his preaching; then David Garrick at the height of his career as an actor; and finally John and William Hunter, those great medical teachers whose influence still lives. The Hunters won Shippen for their own. In anatomy it is clear he found his special bent. For success in this specialty he must have possessed a good degree of hand skill,

acute observational powers, an artist's feeling for structure and form, and a brimming curiosity as to the make-up of the human body and the puzzles of its intricate relationships.

Shippen's experiences in London matured him —and he was there for a longer period than the diary would show, from late November, 1758 until he went to Edinburgh apparently late in the spring of 1760. He had arrived in England to pursue his medical studies just after he had passed his twenty-second birthday, an eager inexperienced young man. In London with the Hunters he acquired his professional stride; a year in Edinburgh completed his medical training with academic finish; he returned to Philadelphia in the first flush of his maturity in 1762 with ambition to set up in his native city such opportunities for medical studies as he had enjoyed to his great professional advantage abroad.

The diary itself will show you from its simple entries the change which was taking place in William Shippen during his London sojurn, and I shall read two series of entries which I have called respectively *Before* and *After* the Hunters, to prove the point. The first series shows him, fresh from Philadelphia and his strict Presbyterian rearing, becoming a follower of George Whitefield and his assistant preachers at the Tabernacle in Moorfields. For professional training Shippen was walking the wards of St. Thomas's Hospital in Southwark that summer; for recreation he was often invited by new friends to the country. In the first series extracted from the main body of the diary, there are entries for five days selected at random.

BEFORE THE HUNTERS

Sunday July 22nd, 1759

One of my most pleasant days, heard in forenoon Mr. Read good old gentleman. Dined with my kind friend Mr. DeBerdt, heard Mr. Elliot in the afternoon who tho a church of England Parson is obliged to turn Dissenter for want of a church. In the evening heard Mr. Kinsman an excellent Preacher at Tabernacle. Supped upon Peas at Mr. D. About 10 went home and bedded at 11½ having spent the day with much peace and pleasure.

Wednesday, August 8th. Rose at 11 to go to Peckham to ride with good Mrs. Huthwaite, to Shooters Hill which commands the most extensive and variegated Prospect perhaps in England, the windings of the River Thames in the middle of so fine a Garden, if I may say so, makes the Prospect enchanting. Dined at Peckham, came home in the Evening upon Mr. Pindar's Horse at 2/6 when I had the great pleasure of seeing London illuminated upon the glorious News of Prince Ferdinand's Victory over the french army and General Wedel's over the Russians, glorious indeed! long live Prince Ferdinand etc! Our Philadelphia illuminations done with more spirit, only 5 or 6 candles being put up [here] against a House that has perhaps 60 or 70 Glasses. Paltry. Saw the mob break 3 Quaker's windows etc.

Thursday 9th. Rose at 8 went to hospital being taking in day. 57 were taken in by Dr. Reeve and Mr. Baker. Walked to Peckham to dine at Mr. Huthwaite's with Mr. and Mrs. Miss and Master Latham, I Hornbuckle, very Merry the whole Time; Miss sings to music prettily; Mast. Latham 8 years old plays well and was never taught by any Body, a natural Fidler. Got home to bed about 10, was put in mind of home much, their family being just like my own in Philadelphia.

Sunday 26th. Rose at 7½ and spent the day as other Sundays at Mr. De Berdts etc. Heard Mr. Godding and Mr. Whitefield.

Monday 27th. Rose at 7 breakfasted with Dr. Fothergill who was very familiar and sociable; paid Governor Hamilton a visit, sat an hour and ½ with him talking over the affairs of the province etc. Very polite and affable. At 11½ o'clock I saw Mr. Cowel cut for the stone very well. At half after twelve went to Dr. McKenzie's touching Lecture and examined 17 pregnant women, etc; Dined at 2½, at 3 went to see Dr. Taylor who is sick. Drank tea with Mrs. Norkitt whose husband is brother to Glover Hunts wife. Spent the evening at home.

These were days of diversified program before he knew the Hunters. Notice how he spends his days in October after dissection has begun at the house in Covent Garden. I have inserted entries from October 5 to October 15, inclusive, in the series entitled

AFTER THE HUNTERS

Friday 5th. Rose at 7, breakfasted at 8½. Dissected till Dinner and after dinner till Lecture Time 5.

Saturday 6th. Rose at 7. Spent the day in the dissecting room till 5; Dr.'s Lecture till 7½; bed at 10½—talking Anatomy with Mr. H. from supper.

Sunday 7th. Dissected all day till 6 walked in the Park till 8 Supped at 9 bedded at 10

Monday 8th. Rose at 7 dissected 7 hours, till Lecture Time at 5, after Lecture operated till 9 (Supper Time) under the direction of Mr. Hunter.

Tuesday 9th. Rose at 6 operated till 8, breakfasted till 9, dissected until 2, dined till 3, dissected

till 5. Lecture till 7, operated till 9, supped till 10 then bed.

Wednesday 10th. Rose at 6 and spent the day as yesterday till 7 at night. Went to coffee house till 9.

Thursday 11th. Rose at 8 dissected till 4 excluding meals. At 5 Dr. Hunter's Lecture till 7. Went to play. [The play given at Drury Lane that evening was The Double Gallant or The Sick Lady's Cure (appropriately enough) adapted from a French farce by Colley Cibber.]

Friday 12th. Dissected till 5 went to see Garrick in Macbeth. Surprising!

Saturday 13th. Dissected till 1. Went to Coffee H. where to my great Joy I found a Pacquet from Philadelphia. Dined with Mr. De Berdt, attended Dr. H. Lecture at 5 till 7½, talked over Anatomy and Lecture till 10 with Mr. Hunter.

Sunday 14th. Rose at 6, dissected till 8, breakfasted, dissected till 1, dined and dissected till 6, wrote until 11.

Monday 15th. Rose at 6, dissected till 12, went with Mr. Hunter to Dr. Middleton's Auction, Lecture at 5, read Sydenham and chirurgical Lectures till 10.

The great interest of Shippen's life was driving him with intensity it is clear to see from these entries. One reason, aside from personal interest in anatomy for this intensive concentration was, of course, the fact that when subjects were obtained for dissection speed became necessary, since space for storage and proper preservatives for keeping the subject in condition were both lacking at this time.

What piques our curiosity especially in this October series of entries is what William Shippen and John Hunter were talking about in their evening colloquies upon anatomy. If only Shippen had imparted to his pages what Mr. Hunter said I should have something electrifying to disclose to you. As it is, my allotted twenty minutes is coming to a close, and I can only hint at the richness of instruction in the Hunterian School of Anatomy conducted for many years by these two gifted brothers, William and John Hunter.

They were a remarkable pair. Their distinction was well realized outside medical circles and those lectures that Dr. Hunter gave were attended oftentimes through special dispensation, and perhaps later by payment of fees, by various important personages keen enough to realize the value of such lectures for the understanding of that curious animal, Man. Horace Walpole's glittering pages refer somewhat disdainfully to the discourses of "the Scotch nightman," William Hunter (a dirty epithet, by the way, and one which was quite undeserved by Dr. Hunter), but there is evidence that somewhat later in the century upon occasion Edmund Burke, Edward Gibbon, Alexander Carlyle, and the Scottish historian William Robertson found Dr. Hunter's anatomical lectures a fascinating experience. When the Rev. Alexander Carlyle, the Dean of Inveresk (nicknamed for his majestic appearance *Jupiter* Carlyle by his friends), was admitted with Robertson by special appointment with William Hunter to a lecture on the eye, he found himself charmed by this excursion outside the field of theology and reported that the lecture was "one of the most elegant, clear and brilliant any of us ever heard." I like particularly the references to Dr. Hunter's lectures in the *Private Letters of Edward Gibbon*. During the winter of 1777 Gibbon could not be induced to leave London under any pretext whatever because, said he, of having undertaken attendance for two hours daily during the season upon Dr. Hunter's lectures "which have opened to me a new and very entertaining scene *within myself*."

The mantle of William Hunter had fallen upon William Shippen's shoulders as he stood on the rostrum in the Philadelphia State House on November 16, 1762 delivering his Introductory Lecture to the first course in anatomy of academic standard ever offered in this country and one which set the pace for future developments in medical education in America. Why did this Introductory Lecture completely disappear? we ask in amazement. John Morgan makes reference to it in his epoch-making *Discourse upon the Institution of Medical Schools in America*, and Shippen himself summarizes an important part of its content in that letter of his to the trustees of the College of Philadelphia in which he applies for the professorship of anatomy and surgery—a letter which in closing I must read in full for the dignity of its expression and for the implication of its restrained statement:

Philadelphia
September 17, 1765

To the Trustees of the College

The instituting of medical schools in this country has been a favorite object of my attention for seven years past, and it is three years since I proposed the expediency and practicability of teaching medicine in all its branches in this city, in a public oration, read

at the State House, introductory to my first course of Anatomy.

I should have long since sought the patronage of the Trustees of this College, but waited to be joined by Dr. Morgan, to whom I first communicated my plan in England and who promised to unite with me in every scheme we might think necessary for the execution of so important a point. I am pleased, however, to hear that you gentlemen on being applied to by Dr. Morgan have taken the plan under your protection and have appointed that gentleman Professor of Medicine.

A professorship of Anatomy and Surgery will be gratefully accepted by, gentlemen, your most obedient and humble servant,

William Shippen, Jr.

HISTORIC BOTANICAL COLLECTIONS OF THE AMERICAN PHILOSOPHICAL SOCIETY AND THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

FRANCIS W. PENNELL

Curator of Botany, Academy of Natural Sciences of Philadelphia

(*Read November 4, 1949*)

If I add the intervening time to my comment of seven years ago, when I had the pleasure of considering with you the career of Benjamin Smith Barton as a naturalist, it is now over twenty-five years that I have "held an unofficial position in this Society, that of Curator of your botanical collections. In 1897, after Thomas Meehan's successful search in your attic for the plants gathered by Lewis and Clark, all such specimens—and they were mostly interesting and old—were deposited at the Academy of Natural Sciences of Philadelphia. Since then, most of these have been mounted, and [now] all have been placed in modern steel storage cases." It was essentially as that remark was being made in September of 1942 that the botanical department of the Academy received the happy addition of the volunteer services of Dr. Edward E. Wildman, and his interest has enabled us to proceed much more rapidly with the selecting and appraisal of our material of historic import. The Academy itself was founded in 1812 and has on its own account collections of about equal age with those of the Philosophical Society. You will see as our story progresses how intricately collections of the two institutions may interlock, and the consequent desirability of a joint consideration of our historic treasures.

These together are the earliest series of plant-specimens that have been preserved in this country, and anticipate by one or more generations the collections at other centers in the United States. Yet they are not the earliest that had been gathered in our territory. Those went to Europe, where most of them are preserved in the greatest of the world's treasure-houses of historic matter, the British Museum. That is the depository of two vast amalgamations of world-wide botanical material, the herbaria of Sir Hans Sloane (1660–1753) and of Sir Joseph Banks (1743–1820); with the former are the plants gathered by John Banister (d. 1692?) in Virginia, by David Krieg (d. 1713) and William Vernon (d. 1711) in Maryland and by John Bartram (1699–1777), our first native American botanist and an original member of this Society, over most of the English American colonies before the Revolutionary War; with the latter are the plants gathered by William Bartram, John's artistic son and author of the famous *Travels*, whose collections were made in the southern colonies just before the Revolution. Also at the British Museum is the series of plants collected by John Clayton (1686–1773) in Virginia and studied by J. F. Gronovius (with the youthful Linnaeus) in Holland; Clayton's own set of these plants was bequeathed to Gloucester County, Virginia, and would be our earliest collection of plants preserved in this country if the county courthouse had not been burned before 1800. Last of all such collections to pass to Europe and quite contemporary with those we are about to consider were the plants collected by the French botanist, André Michaux (1746–1802); he made the American Philosophical Society one of his headquarters, traveling altogether south to Charleston in South Carolina, north to Lake Mistissini in Quebec, and west to the Mississippi River in Illinois; based upon his collections at the Museum d'Histoire Naturelle in Paris is the earliest comprehensive Flora of this country, the *Flora Boreali-Americana* of 1803.

As is well known to all members of this Society, during the last half of the eighteenth and first quarter of the nineteenth centuries Philadelphia was the leading city, both in size and in intellectual life, of the United States. Here the American Philosophical Society had been founded by Benjamin Franklin in 1743, and—a matter of nearly seventy years later—the Academy of Natural Sciences in 1812. I wish to consider the several herbaria, with some account of their collectors or assemblers, that passed to either of these institutions. I want to tell not only the origin and history of these, but also to discuss what may still be

needed for their proper understanding and preservation. It is a curious thing, considering the disparity in date of their beginnings, that the inheriting of such collections seems not to have begun until 1815, when both institutions were in existence; but it is most natural that the herbaria of Muhlenberg and Barton, both of whom died in that year, passed to the well-established and venerable American Philosophical Society and not to the just budding Academy of Natural Sciences with its single room over a baker's shop. But that choice so logical, so almost inevitable at the time, led to a long neglect of your collections from which we are only now fully awaking.

The herbarium of Rev. Henry Muhlenberg (1753–1815), who lived throughout his active life at Lancaster, Pennsylvania, was assembled, both by his own collecting and by specimens received from correspondents, from about 1780 until 1815. The chief of his contributors in this country are listed in the preface to his *Catalogus Plantarum Americae Septentrionalis,* his principal work, which was published at Lancaster in 1813. Other studies, mostly of grasses and sedges, were brought out for him in Germany by the Curator of the Berlin Museum, K. L. Willdenow. In our current number of *Bartonia,* issued by the Philadelphia Botanical Club at the Academy of Natural Sciences, there is a careful study [1] by your member, Dr. E. D. Merrill of Harvard University, telling of Muhlenberg's work as well as considering the (by modern rules of nomenclature) inadequately published names of the *Catalogus.*

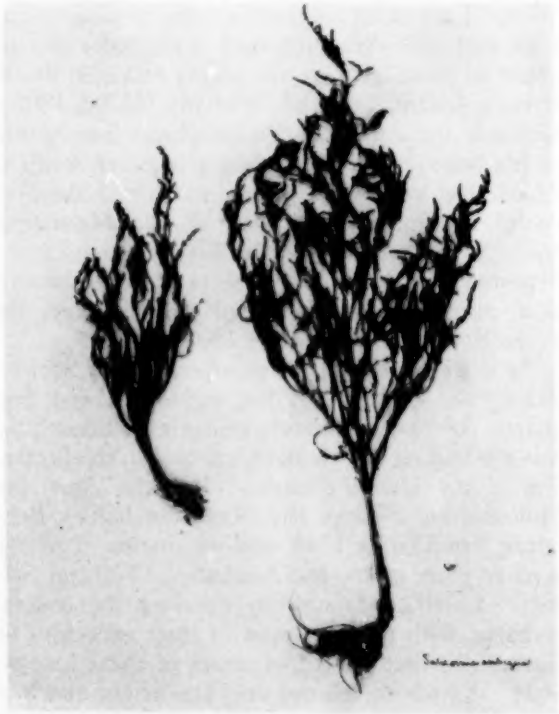

FIG. 1. Muhlenberg's herbarium (A.P.S.): specimen of *Salicornia ambigua* as relabeled by Griffiths.

FIG. 2. Muhlenberg's herbarium (A.P.S.): specimen of *Salicornia indica* Willd., received from describer in Germany, now with annotation by Griffiths.

Muhlenberg's herbarium now occupies two and one-half closely packed cases. As the plants are still unmounted, this means that it contains 4,000 or more specimens that should be provided with all the data that can still be recovered, and then

[1] Merrill, E. D., and Shiu-Ying Hu, Work and publications of Henry Muhlenberg, with special attention to unrecorded or incorrectly recorded Binomials, *Bartonia* **25**: 1–66, 1949.

FIG. 3. Muhlenberg's herbarium (A.P.S.): Muhlenberg's record of specimens in a portfolio of grasses and rushes that passed through hands of W. P. C. Barton. As yet we lack clues to the numbered references to Elliott or under "M," Muhlenberg or perhaps Michaux.

mounted. Unfortunately, there are few memoranda with the plants, but search through the many Muhlenberg manuscripts and letters at your Society, and especially noting who were his correspondents, should reveal much of value. But Dr. William Baldwin, who saw the collection soon after its arrival with you after Muhlenberg's death, expressed in a letter his disappointment at its lack of data, while, since his time, a zealous Curator of your Society, Dr. R. Eglesfield Griffiths, about 1830 put all then at the Society in order by substituting in his own admittedly better handwriting the names of the various species, and seemingly also by eliminating memoranda in Muhlenberg's difficult chirography (figs. 1, 2). The grasses and sedges escaped this by having been purchased by Professor William P. C. Barton, from whom they later passed to your Society (fig. 3); this portion shows us the original condition of the herbarium. In either case all specimens of any species are piled into the same folders together, the result forming a curator's nightmare. For adequate study of this earliest of your herbaria, we need the services of a careful curator for some two years.

We come to more solid ground in the herbarium of Benjamin Smith Barton. The earliest professor of natural history in America and a teacher high in the medical school as well, Barton served the University of Pennsylvania from 1789 until his death in 1815. At the meeting of the Philosophical Society in 1942 I told of his many varied interests and of the enthusiastic temperament that led him to take up project after project only to abandon each in turn.[2] Hence it is that there are so few completed works to his credit. He began a detailed *Prodromus Florae Pennsylvanicae*, of which at least ninety pages were actually printed, although no copy of the work is known to exist. This undertaking is known only by quotations from it in Barton's fragmentary *Flora Virginica*. His chronic sickness (tuberculosis) left him too easily discouraged, and these works seem to have fallen victim to this mood.[3]

Barton's medical training was at the University of Edinburgh, although his doctor's degree came

[2] Benjamin Smith Barton as naturalist, *Proc. Amer. Philos. Soc.* **86**: 108–122, 1942.

[3] See The elder Barton—his plant-collections and the mystery of his Floras, *Bartonia* **9**: 17–34, 1926.

FIG. 4. Barton's herbarium (A.P.S.): isotype of *Eucalyptus piperita*, acquired as indicated on recently formulated label.

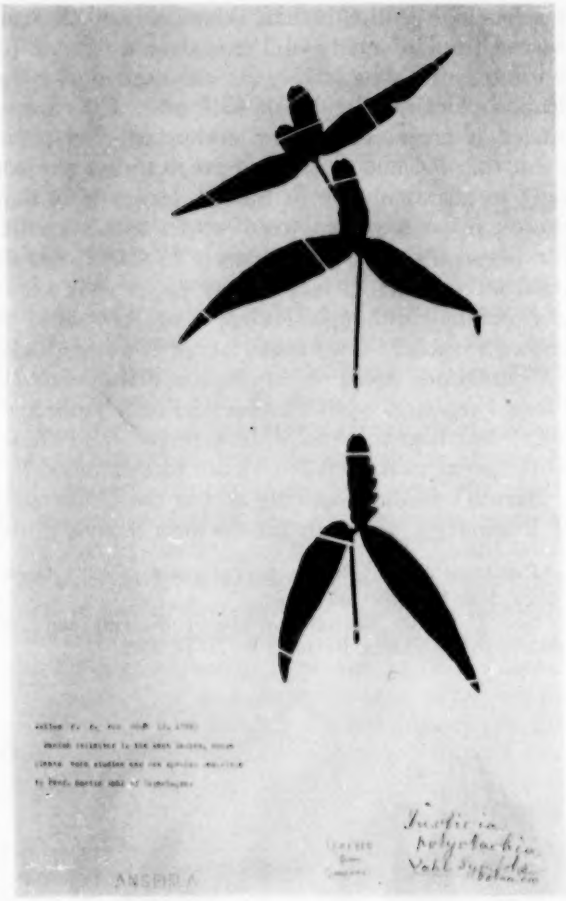

FIG. 5. Barton's herbarium (A.P.S.): isotype of *Justicia polystachia* Vahl, collected by von Rohr who visited Philadelphia in 1791–92.

from the University of Göttingen in Germany.[4] While in England just before his return to Philadelphia in 1789, he evidently acquired a small but remarkably interesting series of specimens. We recently found among your Society's deposits a package marked as pertaining to "Barton's Herbarium"; it contained 27 specimens, each with name of species and some with reference to illustrations in "Sm. Bot. of N. Holl[d]." They proved to be duplicates of the specimens from which James Edward Smith, as yet unknighted but who had already purchased and brought the Linnean Herbarium to England from Sweden and who had just founded the Linnean Society of London, drew up his *Specimen of the Botany of New Holland*, which was published at London in 1793. The plants themselves had been collected in New South Wales in the preceding year, 1788, as may be read in the journal of the collector, Dr. John White, who was Surgeon to the flotilla that carried thither the first convict settlers and that founded the town of Sydney (fig. 4).[5] Clearly the plants had been given to the young American by Dr. Smith.

Among other foreign specimens in Barton's Herbarium from the American Philosophical Society is a series of over one hundred collected in the West Indies and along the northern and eastern South American coast as far as Brazil, by

[4] But see discussion by Dr. Edgar Fahs Smith in "Benjamin Smith Barton, An American Naturalist," an address delivered before the Alumni of the Graduate School of the University of Pennsylvania, February 10, 1916, *Papers Lancaster Co. Hist. Soc.* **28**: 59–66, 1924.

[5] *Journal of a Voyage to new South Wales*, by John White Esq. Surgeon General to the Settlement, London, 1790. This contains dedication, showing that specimens were dispatched home in November, 1788, and also plant-descriptions by J. E. Smith, confirming his having been at work on them in 1789, both for this *Journal* and for his own book of 1793.

FIG. 6. Barton's herbarium (A.P.S.): specimen from Michaux in Barton's book dated 1795.

Julius P. B. von Rohr (*ca.* 1737–1793), a Dane. From 1786 he was collecting plants in this area; in 1791 he sent many to Professor Martin Vahl in Copenhagen; and then he came to Philadelphia, from which he sent to Vahl the original specimens of what the latter described in 1794 as *Gerardia tenuifolia*. But by that time von Rohr himself was lost with the ship on which he had sailed from North America to Guinea. His specimens, seemingly given to Barton while in our city, contain isotypes of various tropical species described by Vahl (fig. 5).

There are still other foreign specimens in Barton's Herbarium, but mostly with vague data and not of much interest. Let us turn to his more ample series of North American plants. The earliest of these are in two books that are marked on the back with the year 1795. These are in main part plants gathered by Barton himself, some from Bartram's Garden, and a few bearing Barton's monogram "B.S.B." so much used on his later collections; there are also 39 other specimens, 23 from John Adlum from Duck Pond and elsewhere "in the back country," 11 from "Andrew" Michaux on his northern trip of 1792 to Lake Mistassini in Quebec (fig. 6), and 5 more from various persons.

FIG. 7. Barton's herbarium (A.P.S.): specimen from trip across central New York in 1797.

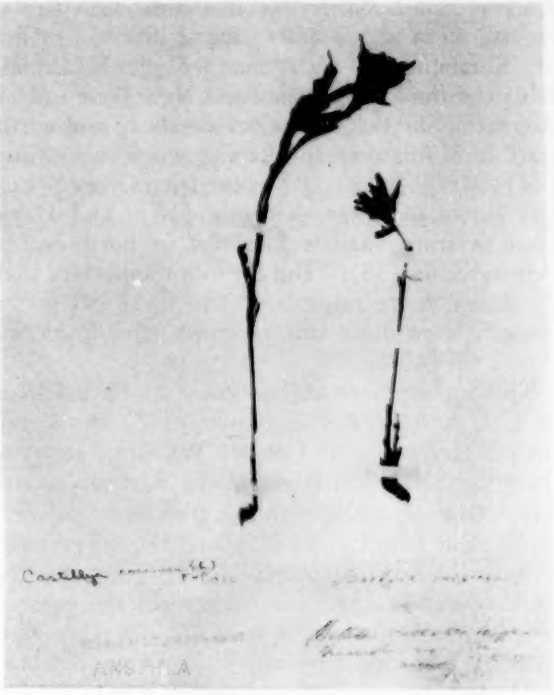

FIG. 8. Barton's herbarium (A.P.S.): specimen from trip across central New York in 1797.

After the appearance of Michaux' *Flora Boreali-Americana* in 1803, Professor Barton set about the preparation of a new and more ample Flora of this country. For this he assembled an herbarium of some 2,000 specimens, those not removed as explained below being now 1,674 sheets. Beside his own collections (144), gathered in 1797 on a journey up the Hudson River and across from Albany to Niagara Falls (Figs. 7, 8); in 1802 on the mountains near Staunton and Warm Springs, Virginia; in 1805 on a nearly circular journey from Philadelphia to Frederick County, Virginia; and in 1806 at Passaic Falls and Perth Amboy, New Jersey, Barton's Herbarium (as stated in my paper of 1926) contains specimens from some thirty other persons. Among these the most considerable are: Mr. "Enslin" [Aloysius

Enslen] from North Carolina and elsewhere in the southern states (69); Dr. [ ] Gray of Southampton, Virginia (71); M[atthias] Kin[n] from Pennsylvania and New Jersey (16); Augustus Shryock from Chambersburg and northward to Mifflintown and Lewistown, Pennsylvania (25); Mr. [ ] Steptoe from Great Seneca and Peaks of Otter, Virginia (15); and David Thomas from various localities in north-central Pennsylvania (58). But far more important than all others, more important than those of Barton himself, were the plants received from Frederick Pursh (1774–1820).

This young German had come to Philadelphia in 1799 and became gardener at "The Woodlands," the estate of Colonel William Hamilton along the Schuylkill River above Bartram's Garden. There Hamilton and his previous gardener, the English John Lyon, had assembled the greatest variety of cultivated plants in this country. (In the Woodlands estate one recognizes the present Woodlands Cemetery, where the old Hamilton mansion is happily preserved.) From his employment there Pursh passed to that of Professor Barton, and, beside becoming practically curator of his herbarium, made for his patron two trips: in 1806 to the mountains of Virginia (fig. 9) and just over the state border to Northampton County, North Carolina, and in 1807 to the lakes of west-central New York and the mountains of Vermont. The routes of both expeditions are easy to follow from the localities of the specimens collected, Pursh being unique among our early botanists in giving precise data of the provenience of his collections. In the Barton Herbarium deposited at the Academy there survive from Pursh's southern trip 895 specimens, from his northern trip 141 specimens, and from localities near Philadelphia, such as Blockley, "Skuilkill," and Woodlands (fig. 10) 113 specimens. The importance of these collections by Pursh, now 1,149 in all but originally likely over 1,500, is seen when we realize that it was Pursh, and not Barton, who carried out Bar-

FIG. 9. Barton's herbarium (A.P.S.): specimen collected by Pursh in Maryland on way to Virginia.

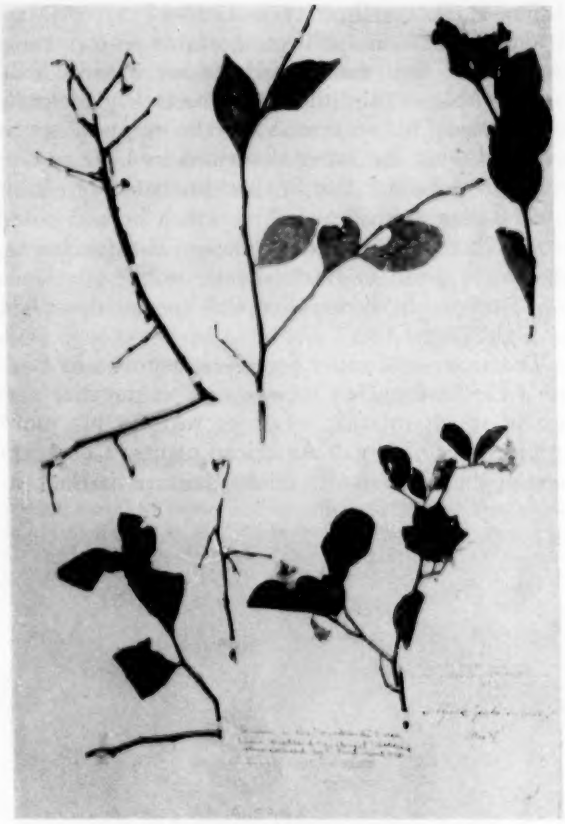

FIG. 10. Barton's herbarium (A.P.S.): specimen gathered by Pursh at the "Woodlands," now Woodlands Cemetery along the Schuylkill River in Philadelphia.

FIG. 11. Barton's herbarium (A.P.S.): specimen collected by Lewis on Lewis & Clark Expedition. One of series found by Thomas Meehan at Society and in 1897 transferred on deposit to Academy of Natural Sciences of Philadelphia, thence loaned for study to Drs. B. L. Robinson and J. M. Greenman at Harvard University, whose identifications, with Pursh's copies of Lewis' labels, were published by Meehan under caption of "The plants of Lewis and Clark's expedition across the Continent, 1804–1806" in *Proc. Acad. Nat. Sci. Phila.* **50**: 12–49, 1898. In same journal, pages 291–315, Elliott Coues gave precise localities to fit dates supplied by Lewis through Pursh.

ton's dream of a second comprehensive North American Flora.

In the winter of 1807–1808 Pursh left his Philadelphia patron and went to New York to have oversight of the Hosack Garden, a project of Dr. David Hosack (1769–1835) which soon after passed to Columbia University and the site of which, that on which Rockefeller Center has developed, is still an important source of revenue to the University. Some three years later Pursh took a voyage to the West Indies, and, soon after his return, went to England. There, with the generous patronage of Sir Aylmour Bourke Lambert (1761–1842), who is said to have had to keep him "under lock and key, with restricted fluid

FIG. 12. Barton's herbarium (A.P.S.): specimen collected by Lewis on Lewis & Clark's Expedition. Another of the same series as in figure 11, but only with identification by Thomas Meehan, and with original note (on purple paper) by Meriwether Lewis as well as generalized summary by Pursh.

FIG. 13. Lambert's herbarium (A.N.S.P.): type of *Berberis aquifolium* Pursh, Oregon Grape, now state flower of Oregon. Shows Pursh's copies of Lewis' labels or data, and reveals that Pursh first thought of naming this, rather than the Bitter Root, for Lewis. Before 1842 was in herbarium of Sir Aylmour Bourke Lambert, as is evidenced by Lambert's record on the reverse reading "N. America Herb: Lewis & Clarck —Fred: Pursh.," and the dated water-mark "1811."

stimulation,"[6] he wrote his *Flora Americae Septentrionalis;* its appearance in 1814 marked a distinct advance over the earlier work of Michaux.

Now, Pursh had carried to England for the preparation of this Flora many collections from the herbarium of Professor Barton, and so, since Barton was Vice-president of the American Philosophical Society, we may consider them as really borrowed from this Society. Such must have been the understanding about the plants collected by Meriwether Lewis on the return course of the Lewis and Clark transcontinental exploring expedition. Of this series, transmitted supposedly by President Jefferson, there survive in Barton's Herbarium 182 sheets of specimens. Like other plants in Barton's Herbarium, these show evidence of Pursh's attention, he having copied Lewis' labels; fortunately, knowing Pursh's characteristic care, we may be confident that little or no information has so been lost (fig. 11). There still remain various notes, as to uses of the plants and other matters, that survive on purple labels bearing Lewis' own handwriting (fig. 12). Evidently Pursh was planning to study the Lewis and Clark plants, and evidently—I presume with the full approval of Professor Barton—Pursh carried to England for this purpose specimens of the Lewis and Clark (fig. 13) as well as of the strictly Barton series of plants.

Why he should need to do so is evident when we reflect that there was no good herbarium in the United States by which Pursh could check his material. So he either took abroad actual specimens or else distinctive descriptions that picked out salient points. He studied his material at Boyton, Lambert's estate near London. But, after the publication of the *Flora Americae Septentrionalis* in 1814, why were these specimens not returned to Philadelphia, to Professor Barton or at least to the American Philosophical Society? His contemporaries spoke of Pursh's shady dealings, and certain it is that the preserved collections of his trips do not support his claim of having visited either the high mountains of Carolina or those of New Hampshire, as asserted in the introduction to his *Flora.* However that be and thanks to the power of checking due to the use of paper with dated watermarks, by the time that Pursh's *Flora* has been published, the specimens from your Society had already been mounted for the Lambert Herbarium on sheets of paper watermarked "1811." Pursh left these specimens with Lambert, and returned to America to become a Canadian citizen, dying in Montreal in 1820. Sir Aylmour lived on until 1842, when, after his death in January, his vast herbarium was sold piecemeal at auction and widely scattered. But of the American plants that Pursh studied, a goodly proportion, presumably the majority, did return to Philadelphia, and were quietly incorporated into the herbarium of the Academy of Natural Sciences. This is hardly where they should have gone, but those

[6] Quoted from M. L. Fernald's delightful account of Some early botanists of the American Philosophical Society, in *Proc. Amer. Philos. Soc.* **86**: 63-71, 1942 (ref. to p. 66). Pursh is said to have been a habitual drunkard, but, as we shall see in our comparison of him with Nuttall and others, he was the most logical and careful botanist of his time dealing with our American plants.

plants that Pursh studied, whether in his American or his English days, are now preserved in adjoining cases in the same room.

The acquisition of such a valuable portion of the Lambert Herbarium was doubtless due to the great mid-nineteenth century donor to the Academy, Dr. Thomas B. Wilson. I have trailed him sufficiently to know that he was in England in the summer of 1842, while it is legend at the Academy that Wilson insisted that all his donations remain anonymous—if anything were said about them, the donations would cease. The Lambert specimens were scattered to their proper places in the systematic sequence of the herbarium, but now Dr. Wildman is on the quest of all significant historic quarry in our herbarium, searching case by case through some hundreds of thousands of mounted sheets. When he shall have assembled all that we have of these Lambert sheets, then will begin our critical examination both of them and of the Philosophical Society's sheets in the Barton Herbarium, the search being for the actual types, the very specimens studied by Pursh for his many descriptions of new species in the *Flora Americae Septentrionalis*. Those on the Lambert sheets will be easily recognized, for they should bear Pursh's newly assigned names. The sheets, mostly still in their original form, will be characteristic, narrow and seen against the light finely cross-lined; they will be watermarked with the year of manufacture; on the reverse in the upper left-hand corner, Lambert will have written a general memorandum of the collection's source, while on the front the label will be either in Pursh's handwriting or at least the identification so written. But if the plant be one of those in the Barton Herbarium still waiting to be supplied its name, the task involves its identification with Pursh's own description that was actually made from it. You see how valuable to American botanists, who have too long felt that Pursh's types had somehow disappeared in the course of time, will be the completion of this curatorial task. Even after the plants of both series are all available for the work, I suspect that the task will demand more than a year's careful study.

Returning now to Professor Barton, we must take up the career of another botanist, also originally from Europe, who likewise developed in our city with help from the University professor. Elsewhere I have traced in detail the fascinating life of our most-travelled early botanist, Thomas

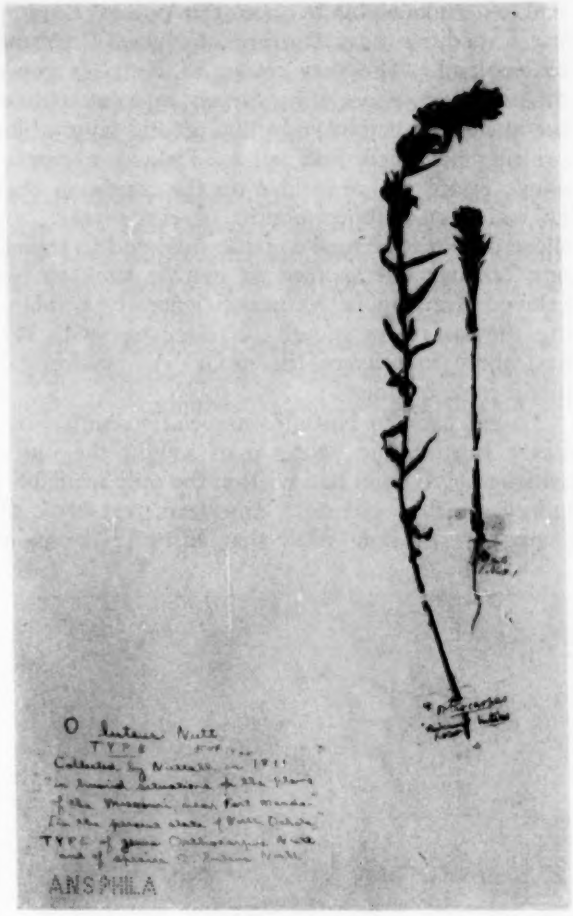

FIG. 14. Nuttall's herbarium (A.N.S.P.): type of genus *Orthocarpus* and of its generitype, *O. luteus*. Nuttall's published statement locates the specimen as from near Fort Mandan in the present North Dakota. Note meagreness of this type specimen and of its companion labeled "R. Mts.," the latter statement meaning that Nuttall had gathered it anywhere between western Nebraska and southern Idaho, in 1834.

Nuttall (1786–1859).[7] It proved surprisingly different from the generally accepted "Biographical Notice" of him that Elias Durand read to your Society soon after Nuttall's death in England and which was printed in your *Proceedings* in 1860, but happily my findings agree in all details with an unsigned "Biographical Sketch" that had appeared five days earlier in the *Gardener's Monthly*. Thomas Meehan, then a young immigrant from Ireland, clearly drew his information from someone who knew Nuttall well; my presumption is that his informant was Dr. Charles Pickering, who

[7] Travels and scientific collections of Thomas Nuttall, *Bartonia* **18**: 1–51, 1936.

had left Philadelphia to go on the Wilkes' Exploring Expedition near the time of Nuttall's retiring to England. The very reason of Nuttall's going fits into the romance of his career; he went because the uncle who had brought him up and taught him printing had died, and left to Thomas a gentleman's estate in Lancashire on the condition that he reside on it nine months of every year. In those days this seemed to put a final end to travel, but Thomas did succeed in getting back to his beloved Academy of Natural Sciences by combining the last three months of one year with the first three months of the next. An unseasonal plight for a botanist!

To get back to Nuttall's association with Professor Barton, the young man sought the older because Dr. Barton had written the only available, indeed the first and only American, text-book of botany. (We now know that Nuttall's devotion to botany had started in his youth in England.[8]) His coming to America and meeting Barton was in 1807, and two years later Barton sent Nuttall south to Sussex County, Delaware; his collections are still preserved in a bundle to themselves among the Barton plants from the American Philosophical Society, and in 1940 Dr. Robert R. Tatnall presented in *Bartonia*[9] a letter from Nuttall to Barton and gave some account of the specimens obtained.

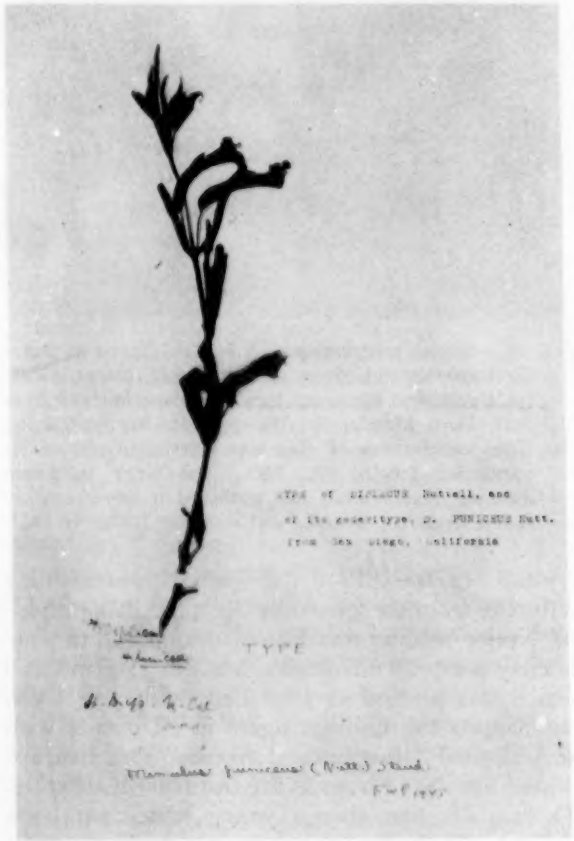

FIG. 15. Nuttall's herbarium (A.N.S.P.): type of genus *Diplacus* and of its generitype, *D. puniceus*. Locality is San Diego, Calif., "N. Cal." designating "Nueva California" for the mainland in contrast to the peninsula, California itself. Collected by Nuttall in 1836.

It was in 1810 that Nuttall started forth on a two years' expedition with expenses to be paid by Barton and for which he was to receive in addition eight dollars a month. In the wonderful discovery of Professor Barton's personal papers at "Montgomery Place," Annandale-on-Hudson, New York, about which the late Violetta W. Delafield tells so vividly,[10] there survive the very terms of agreement, signed by both, for this journey, as well as Barton's copy of his "Directions for Mr. Thomas Nuttall." Suffice it to say that the young man was to pay nearly as much attention to Indians as to plants, that he was to go to Lake Erie, thence by trail (with Indian guide) from Detroit to Chicago, thence across the Winnebago and Menominee country to Lake Superior, and then northwest by Lake "Winnipic" on to Beaver River in latitude 55° N., and to winter at some British factory (presumably Hudson's Bay Company); returning, he was to go southeast to the Missouri River, down it to about the Pawnee villages, thence eastward to the upper waters of the Mississippi River, down stream by the Falls of St. Anthony (the present Minneapolis) to the mouth of the Illinois River, and so home by Fort Vincennes on the Wabash. What a program for a young enthusiast, even if there had not been the Sioux Indians directly on the course! What Nuttall did was to go to Detroit, there evidently discovering that any economic travel must be by canoe rather than overland (he was encumbered by a trunk), thus on to that great meeting-place of waterways, Michilimakinac, where he was when the Astoria Expedition (about which Washington Irving wrote his book, *Astoria*) passed that point (from Montreal via the Ottawa River). Evidently he was here invited to join it, for we find him in 1811 journeying with it up the Missouri River. (As also was John Bradbury, a fellow Englishman interested

[8] An English obituary account of Thomas Nuttall, *Bartonia* **19**: 50–53, 1938.

[9] Nuttall's plant collections in southern Delaware, *Bartonia* **20**: 1–6, 1940.

[10] The finding of the Barton Papers, *Bartonia* **18**: 51, 1936.

in plants, who had spent the year 1810 near St. Louis, and was evidently there invited by Captain Hunt of the Astoria party; Bradbury's plants reached England before him, were there studied by Pursh, and through the Lambert Herbarium a set is now at the Academy of Natural Sciences of Philadelphia.[11]) Nuttall reached the present North Dakota (fig. 14), returned to St. Louis, early in 1812 descended the Mississippi to New Orleans, whence, finding war brewing between the United States and England, he prudently returned to his own country. When war was over in 1815, he brought his Missouri specimens back to Philadelphia, presumably to give to Barton; but Barton died before the end of that year and the specimens passed to the Academy of Natural Sciences, where Nuttall worked (and also passed his nights) during the preparation of his *Genera of North American Plants,* published in Philadelphia in 1818. To the Academy came also plants from Nuttall's many later trips: in 1815, on his way back from England, through the southern states from Carolina to the Ohio River; in 1819 to the Arkansas Territory, our southwestern frontier, a journey for which Nuttall's own journal has been published; in 1830 to Florida; and from 1834 to 1836 on his transcontinental journey westward with the ornithologist John K. Townsend (whose journal has been published) to the Oregon country and on to the Hawaiian Islands, thence in 1836 to California (fig. 15) from which he returned via Cape Horn on the same boat with the seaman, Richard Henry Dana, whose *Two Years before the Mast* pays due respect to the ship's supercargo, "Old Curious." But increasingly as the years went on, Nuttall reserved more and more material for his own herbarium, until on his recall in 1841 to be a landed gentleman in England, a large herbarium went with him that on his death in 1859 passed to the British Museum in London.

As to Nuttall's specimens, they form a sad contrast to Pursh's. Presumably due to the exigencies of transportation they are apt to be peculiarly small, while the data are correspondingly meagre. Instead of Pursh's exact localities, "Ark" was sufficient for a plant gathered in Arkansas Territory, "Missouri" for anywhere along the Missouri River, and "R. Mts." was applicable to anything gathered across what was to become Wyoming and Idaho. But in publishing Nuttall gave so much fuller data, as to lead one to surmise that he was drawing upon record-books that have not survived. On seeing his own herbarium when in London in 1930, I learned that no record-books had come with Nuttall's plants. Nor did his own specimens have any fuller data than had been put on the little slips that accompany those at our Academy. Both have little squares of paper, slit so as to pass the stems through them, and all names of new genera or species are indicated by asterisks before them. Because of his sudden return to England, both the Academy's herbarium and that of the British Museum contain a host of his indicated and named, but unpublished, species.

FIG. 16. Schweinitz' herbarium (A.N.S.P.): specimen collected by him near Salem, North Carolina, where his manuscript "Flora Salemitana" (p. 86) records it as "Very com[m]on in our fields." Schweinitz wrote directly on the folded sheets of soft paper in which he placed his plants. Academy label at bottom right bears handwriting of John H. Redfield (1815–1895), who from 1876 saw to the mounting of its botanical collections.

[11] A characteristic error of Durand is to have Nuttall accompany Bradbury by invitation from Philadelphia, and then to have him return directly to Philadelphia, presumptions contradicted both by William P. C. Barton's obituary sketch of his uncle Benjamin and incidentally in various places by Nuttall himself.

Older than Nuttall by seven years was Dr. William Baldwin (1779–1819), from Chester County, Pennsylvania. He also was of the Barton tradition, by having been, along with his Chester County friend, Dr. William Darlington (1782–1863), the author of the *Flora Cestrica*, a pupil of Professor Barton at the University of Pennsylvania. Baldwin settled as a physician at Wilmington, Delaware; then for his health's sake made long sea-voyages to South America and to China; then for some years was at an army-post in southern Georgia, collecting along the border-river St. Mary; and lastly started out on Long's Exploring Expedition to the Rocky Mountains, but on the way succumbed to tuberculosis in Missouri. Both the American Philosophical Society and the Academy have specimens of his, the former being a small series of the sedge-genus *Rhynchospora* and perhaps other southern plants, and the

FIG. 17. Schweinitz' herbarium (A.N.S.P.): isotype of *Pedicularis gracilis* Wall., from the Indian Himalayas. Schweinitz has discarded Wallich's label, and inscribed on the sheet of soft paper his own memoranda of the plant's name and collector.

FIG. 18. Schweinitz' herbarium (A.N.S.P.): paper enfolding specimen inscribed by Schweinitz with names of species, country, and person who gave him the plant. Original label has been discarded, but presumably Hering got plant from Weigelt. Name given to species proves erroneous, the plant not being *Gardoquia* of the Menthaceae but the recently described *Achetaria guianensis* of the Scrophulariaceae.

latter his own considerable herbarium which by some unlucky chance passed to Schweinitz before reaching its real goal, the Academy. It was treated in usual Schweinitzian fashion.

If Nuttall's specimens disappoint us, what must be our reaction about those of Lewis David von Schweinitz (1780–1834)? This administrator of the estates of the Moravian Church seems to have been a delightful person, "in social life he was the center of every circle which he entered," his industry was amazing, his herbarium, estimated to contain about 23,000 species of plants, was hailed as admirable, and on its arrival at the Academy of Natural Sciences after Schweinitz' death, the Academy's series of plants was simply added to it, the total making about 28,000 species in all. Schweinitz' greatest achievement was his work

on Fungi. Muhlenberg had earlier gathered many specimens of these, as well as of mosses and other flowerless plants. But he was far excelled by the zeal of Schweinitz, who published his great catalogue, listing 3,098 species, largely of his own describing, in 1832.[12] In another and less formal way Schweinitz showed his habitual busy-ness, and the Academy has a succession of large record-books in which he entered by hand all specimens in his herbarium. There has also survived at the Academy a manuscript "Synopsis Plantarum Americanarum," Schweinitz' own Flora, in Latin, of the flowering plants of the eastern United States.

If Nuttall was vague about telling the sources of his specimens, Schweinitz was less obviously, but more deceivingly, so. From 1812 to 1821 he lived at Salem, North Carolina, and from 1822 to 1834 at Bethlehem, Pennsylvania; his plants collected in North Carolina were simply labeled "Salem" or "Sal" (fig. 16), while anything gathered over much of eastern Pennsylvania was similarly marked "Beth." But worse than his treatment of his own collections was what habitually happened to all Schweinitz received from others. He uniformly discarded all original labels, simply writing on the soft sheets of paper, in which he placed the specimens, a brief memorandum of collector and country, and often any number that he found assigned to the collection. As Schweinitz' correspondence was world-wide, the Academy has good representatives of many far-away series,[13] such as Ledebour's plants of the Altai Mountains in central Asia, of which that botanist was writing a *Flora Altaica*; Wallich's plants of India, of the Flora of which country he wrote a *Catalogue* (fig. 17); Hering's plants from Surinam, actually collected by Christoph Weigelt (fig. 18); Leitner's plants from "C. B. S." (Caput Bonae Spei, the Cape of Good Hope)—Leitner is suspected to have been a fellow Moravian—, etc. But now, with only Schweinitz' notes of labels before us, how ardently we wish that we had today the data that was so wantonly thrown away.

We now come to the end of our historical progress through the remarkable field of early Pennsylvania botanists; but there are other collections, especially at the Academy of Natural Sciences, that are fully as old as those of which we have been telling, but concerning how they come to be in Philadelphia we know nothing.

Perhaps the earliest of these are a few plants from John Reinhold Forster (1729–1798), who was botanist on Captain James Cook's second voyage from 1772 to 1775 (fig. 19). Dr. Wildman, who has now gone about two-thirds of the way through the Academy's herbarium, has picked out 23 sheets collected by Forster or his son, all being from the Southern Hemisphere.

FIG. 19. Forster plants (A.N.S.P.) : isotype of *Casuarina nodiflora* Forst. f., described by Georg Forster (1754–1794), who had accompanied his father J. R. Forster on Captain James Cook's Second Voyage, and who published this in 1786 in his own *Florulae Insularum Australium Prodromus*. *Embothrium* (mentioned on accompanying slip) had been published in 1776 in *Characteres Generum Plantarum, quas in Itinere ad insulas Maris Australis collegerunt, descripserunt, delinearunt annis 1772–1775*, a work produced jointly by father and son.

[12] Synopsis Fungorum in America Boreali media degentium, *Trans. Amer. Philos. Soc.* **4**: 141–316, 1832.

[13] One of Schweinitz' great manuscript catalogues contains a list of 108 "Contributores" to his herbarium. This is fuller than the earlier-found list from which Dr. John H. Barnhart prepared his paper on "The Botanical Correspondents of Schweinitz," that was published in *Bartonia* **16**: 19–36, 1935.

A much larger number of plants, actually to be counted by the hundreds, are those at the Academy that were collected by Francis Masson (1741–1805–6) in South Africa. They bear labels with good field data, these presumably in Masson's handwriting; and also slips in Nuttall's hand reading "C. B. S. Masson" (fig. 20). From Britten and Boulger's *Biographical Index of deceased British and Irish Botanists* [14] we learn that Francis Masson was the first collector ever sent out by Kew Gardens, that he was at the Cape in 1772–1773 and again at the Cape and in the Interior in 1786 to 1795. During this latter period he was with the Swedish botanist, Carl P. Thunberg, who was Linnaeus' pupil and (after Linnaeus' own son) his successor at the University of Upsala, and who had the distinction of writing the first *Flora Capensis*, Flora of the Cape Region of South Africa. Unfortunately, our many specimens from Masson crossed the ocean before being named in Europe, but it is evident enough that their careful identification would reveal isotypes of many species subsequently described from duplicates in Europe, and also actual topotypes of Thunberg's species as well. For this curatorial task a good taxonomist would require one to two years' time.

[14] Second edition, revised and completed by A. B. Rendle, 1831.

FIG. 20. Masson plants (A.N.S.P.): specimen from South Africa presumably gathered between 1786 and 1795.

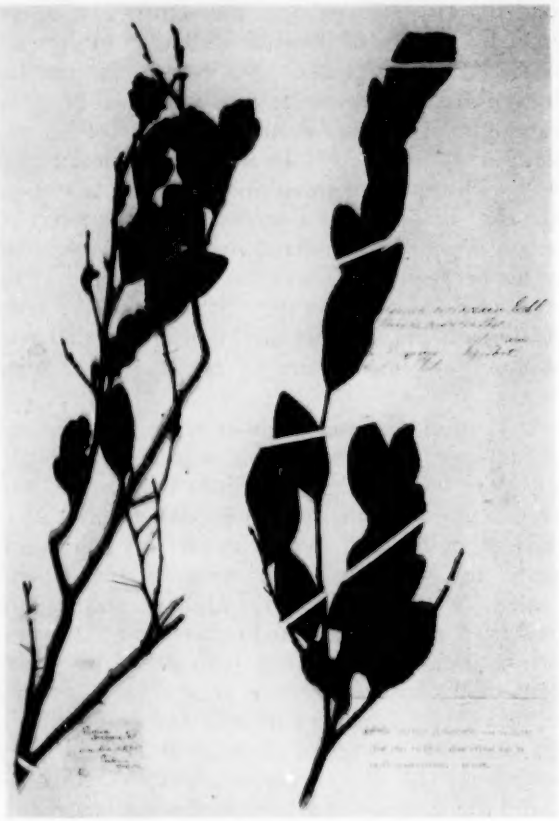

FIG. 21. Poiteau plants (A.N.S.P.): isotype of genus *Rumea* and of its generitype, *R. coriacea*.

Lastly, I must mention Antoine Poiteau (1766–1854), who was in Haiti from 1796 to 1800, when he left because of the negro insurrection that drove out the French. Although he, with P. J. F. Turpin who had been with him in Haiti as artist, later published together various works on the flora of France (including a Flora of the environs of Paris), Poiteau seems to have done too little with his Haitian collections. He did publish a new genus, *Rumea*, with a new species, *R. coriacea*, at Paris in 1815 [15] (fig. 21). The Academy has several hundred specimens from Poiteau's work in Haiti, although they are all credited to "St.

[15] *Mem. Mus. Paris.* **1**: 62, pl. 9, 1815.

Dom.," Santo Domingo, I suppose intending that term for the whole island and not merely for its eastern portion.

Such are the main collections about which I have wanted to tell you. After 1841, with the loss of both Nuttall and Pickering, botany languished in Philadelphia. The middle nineteenth century was the period of a gentle, but masterful, dictatorship in the botanical taxonomy of this country. First, John Torrey (1796–1873) of New York, and then, more completely, Asa Gray (1810–1888) of Harvard—men whose thoroughness did outshine any of the earlier American workers—came to be regarded as having almost unerringly solved essentially all our problems. William Darlington, whose *Flora Cestrica* of 1837 had been full of pertinent observations, withdrew nearly all in 1853 that did not accord with the new authority. Elias Durand, the pharmacist, who acted as Curator of the Academy's botanical collections as well as of those of the American Philosophical Society, made for the Academy a special "Herbarium Boreali Americanum" of one or a few specimens of each species, so that, even though unmounted, they might be speedily consulted; other specimens were then thought superfluous and might be given away. He gave a set of Nuttall's plants to Dr. Gray in 1866 [16] with the Philadelphia-effacing comment: "I thought these original species and genera were owing to the herbarium of Dr. Gray, which was to be the standard herbarium of the North Amer. plants." Today we have a much better idea of the work in botanical taxonomy still to be done than had our predecessors of eighty years ago, and of the importance of preserving, and in a clearly referable condition, all the specimens that can show us precisely what were the species and subspecies of earlier botanists. From the time of Torrey forward, types at many institutions may be readily found, but those of the earlier men, including the botanists of whom I have been telling, need a clearer record.

In the cases of Muhlenberg, Barton, Pursh, and Masson I have stated that much further study is needed before we can positively indicate the types or isotypes of their various species. In the case of such a man as Masson it will require a published statement to let the fact be known that there are these available to a person in this country. The same remark applies to many from whom Schweinitz received specimens. As the whole matter lies in half-measure with the American Philosophical Society, I am hoping—as in so many other projects where its interests were not so intimately concerned—that we can look to the Society for assistance. My suggestion is that an able botanist, a taxonomist with historical interest, be given an appointment for some years (five perhaps), to curate these collections and make an adequate report upon them. He would fill out our knowledge of the historic background, but—much more helpful to the practical taxonomist of this country—he should, after the necessary careful study, make an inventory of the types and isotypes that are in our possession. Until such a record is made, botanists will continue to search abroad for historic specimens that really are, wholly or in part, right here in Philadelphia.

[16] Chase, Agnes, The Durand Herbarium, *Bartonia* **17**: 40–45, 1936; and also the preceding account by Pennell, F. W., Elias Durand and his association with the Academy of Natural Sciences of Philadelphia, *Bartonia* **17**: 33–39, 1936. Durand tells about purchasing specimens from Nuttall, whose acquaintance he made about 1833, and of the trade that he made with Dr. Gray for specimens that to Durand would be of equivalent value; never does it appear that "as patron and close friend, Durand had his pick of Nuttall's materials," as told us by Professor Fernald in *Proc. Amer. Philos. Soc.* **86**: 67, 1942.

SOME APPROACHES TO THE STUDY OF THE ROLE OF METALS IN THE METABOLISM OF MICROORGANISMS

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(Read in part November 4, 1949)

THE recent spectacular successes which have attended research on the role of organic factors such as vitamins and hormones in life processes have not been matched by corresponding advances in our comprehension of the inorganic basis of living matter. In general, the effort made in this area is far less than that which has been devoted to the organic field. Yet an intensive cultivation of this almost fallow field of investigation may be expected to yield quite as rich rewards, both in a practical and in theoretical sense. On the first score, the implications are very broad. The practical significance of a better understanding of the fundamentals of the inorganic nutrition of plants for human feeding under the ever-increasing pressures of population is obvious. Its practical significance for the nutrition of secondary sources of human food such as livestock is equally evident. It is less commonly recognized that a third significance of the nutrition of food animals springs from our utter dependence upon livestock as the source of many substances of prime importance for both health and biochemical research—such compounds as insulin and the hormones of the adrenal cortex, to name but two examples.

On the theoretical side, studies of the role of inorganic factors in metabolism have paved the way for a deeper understanding of living processes in general. A specific case, among a number to be discussed, is the role of cobalt in the construction of nucleic acids, and possibly in the fabrication of the gene itself, through the occurrence of cobalt in vitamin B_{12}.

This paper will survey broad strategy and some of the detailed techniques for the investigation of the role of inorganic factors in microbial metabolism and will include a discussion of some promising avenues of research in this field in the light of recent developments in this laboratory and elsewhere.

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COBALT AND VITAMIN B_{12}

The discovery that the antipernicious-anemia factor (vitamin B_{12}) contains cobalt (1, 2)[3, 4] provides a superb example of the interplay of theory and practice in working out the role of trace elements. It has long been known that animals pastured on the herbage of certain soils sicken unless cobalt is added to these pastures (3). However, attempts to induce artificial deficiencies in organisms other than ruminants have failed, and a belief grew that cobalt is required only by ruminants (4). The fact that B_{12} is required also by the rat (5), chick (6), certain lactobacilli (7), and several algal flagellates (8),[5] suggests, however, that B_{12}, and, by extension, cobalt, may well be constituents of all living matter. Surprisingly, B_{12} as measured by rat or chick assays is virtually absent in green plants and yeast; in fact, the rations for inducing B_{12} deficiency in poultry and the rat depend on plant sources of protein. The "animal protein factor" which frequently is the limiting factor for the growth of animals on plant diets contains B_{12} as an outstanding component,[6] and emerges as an important cost item in the corn and hog agriculture of the Middle West, and in industrialized poultry-raising.

It is unlikely that there is a complete deficiency of B_{12} in plant products: the observation that the algal flagellate *Euglena gracilis* requires an external source of B_{12} hints at the presence of B_{12} in land plants. A test of the effectiveness of *Euglena* cells in relieving B_{12} deficiency in the chick and rat might decide whether the ostensible lack of

[3] Numbers in parentheses refer to references at the end of the article.

[4] It is likely that B_{12} is required by normal humans and that the heightened need exhibited by pernicious anemia patients reflects their inability to digest and absorb B_{12} in the diet.

[5] In unpublished experiments (Provasoli, L., I. J. Pintner, and S. H. Hutner) a strain of *Chlamydomonas* was found which required B_{12}.

[6] Additional unidentified factors may play a part (6).

TABLE 1

MEDIUM FOR ASSAY OF VITAMIN B_{12} WITH *Euglena gracilis*

	grams		milligrams
KH_2PO_4	0.05	Ca (as NO_3^-)	2.5
$MgSO_4 \cdot 7H_2O$	0.05	Zn	1.0
L-Glutamic acid	0.5	B	2.5
DL-Malic acid	0.1	Mn	0.5
		Fe	0.5
		Cu	0.05
		Mo	0.01
		Co	0.02
		Thiamine	0.1

Distilled water to 100 ml. No adjustment of pH necessary (pH approximately 3.3–3.7).

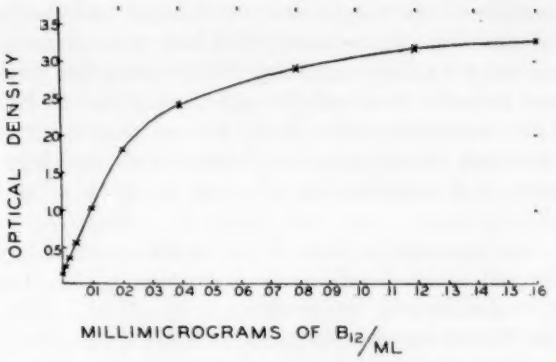

FIG. 1. Assay of vitamin B_{12} with *Euglena gracilis*.

B_{12} in green plants reflects the rapid conversion there of B_{12} to metabolites lacking B_{12} activity for animals.

By using *Euglena* as the experimental object, B_{12}-cobalt relations can be examined quantitatively, and the difficulties in inducing primary cobalt deficiencies in plants or animals in the laboratory can be examined under favorable conditions. With the aid of an assay medium of the type shown in table 1, which allows a more sensitive response to B_{12} than does the medium first described (8),[7] the number of B_{12} molecules required for the development of an individual *Euglena* cell may be determined together with the number of cobalt atoms required in the form of B_{12}.

In the linear portions of the curve of growth of *Euglena* as a function of B_{12} (fig. 1) 0.01 millimicrogram of B_{12} allowed the formation of 880,000 *Euglena* cells. Thus the weight of B_{12} required to form an individual *Euglena* is $\frac{0.01\ \text{m}\mu\text{g}}{880{,}000} = 1.13 \times 10^{-17}$ grams. Using 1,400 as the value for the minimal molecular weight of B_{12} (9), the number of B_{12} molecules per *Euglena* ("n") is given by the relation:

$$\frac{n}{\text{weight of } B_{12}/\textit{Euglena} \text{ cell}} = \frac{6 \times 10^{23}}{\text{Molecular wt. of } B_{12} \text{ in grams}}$$

(where 6×10^{23} is Avogadro's number, the number of molecules in a gram molecular wt.). Substituting numerical values from the experiment:

$$\frac{n}{1.13 \times 10^{-17}} = \frac{6 \times 10^{23}}{1{,}400}$$

(using 1,400 as the minimal molecular weight of B_{12}). Thus $n = 4{,}900$ molecules of B_{12} as the number required for the formation of a single cell of *Euglena*.

It will be seen that the amount of cobalt contained in the B_{12} supplied *Euglena* is extremely small. This can be graphically demonstrated in the following fashion:

Maximum growth of *Euglena* (about 7,000,000 *Euglenas*/ml. of culture medium) requires about 0.15 mμg of B_{12}/ml., or 1.5×10^{-10} grams/ml. of culture medium. Taking the proportion of cobalt in B_{12} to be about 4 per cent by weight (1, 10), it follows that the concentration of cobalt required is $1.5 \times 10^{-11} \times 0.04 = 6 \times 10^{-13}$ g. cobalt/ml. of culture medium. The concentration of iron that must be supplied in this medium is about 2×10^{-6} grams/ml. The iron supplied was purified by vacuum distillation for use as a spectroscopic standard (Westinghouse) and carried a cobalt contamination of 0.001 per cent (analysis furnished by Westinghouse). Therefore the cobalt furnished to the medium with the iron is $(2 \times 10^{-6}) \times (1 \times 10^{-5}) = 2 \times 10^{-11}$ grams/ml. It follows that the ratio of $\frac{\text{cobalt supplied}}{\text{cobalt needed}} = \frac{2 \times 10^{-11}}{6 \times 10^{-13}} = 33$. Iron is quantitatively a minor ingredient of the culture medium. Yet in this instance, even when it was required only in small quantities, and where it was supplied in a form so pure that the cobalt contamination was barely de-

[7] It is instructive in contrasting the convenience of microbiological *vs.* animal assay methods for B vitamins, to compare the medium of table 1 with the basal ration for the chick ration of B_{12} (6). The great disadvantage of microbiological assay methods is the very limited ability of the assay organism to utilize a vitamin bound to a high molecular weight compound; the animal has a battery of digestive enzymes to facilitate the absorption of the vitamin. Work has barely started on how to liberate microbiologically active B_{12} from high molecular weight conjugates.

tectable by the most sensitive physical and chemical methods, the amount of cobalt present as an impurity was approximately thirty times the minimal amount required for the formation of B_{12}. This underlines the need for extremely pure chemicals in this type of investigation, and highlights a fruitful source of error in work of this kind.

The assumption that *all* the cobalt consumed by the cell is involved in the formation of B_{12} has been challenged by Becker *et al.* (10). They found that the symptoms of cobalt deficiencies in lambs were not relieved by the parenteral administration of doses of vitamin B_{12} (120 μg over a period of 6 weeks), whereas deficient lambs fed 1 mg. cobalt per day quickly recovered. The counterpart of this experiment for *Euglena* would be to devise a cobalt-free medium, and then to determine whether the cobalt bound in the B_{12} supplied had to be supplemented with free cobalt ion. A necessary assumption in this line of experimentation is that the cobalt in the B_{12} molecule remains completely bound and is not liberated into the surrounding medium as ionic cobalt. This ionic cobalt might be taken into the cell less efficiently than cobalt bound in B_{12} and would thus increase the apparent cobalt requirement. The associated problem of the tightness of binding of cobalt in B_{12} may be attacked by determining whether radioactive cobalt in B_{12} exchanges with ionic cobalt in the external medium, or *vice versa*. The nature of the cobalt link in B_{12} is unknown, other than that cobalt is coordinately bound. If cobalt is bound in a porphyrin-like compound, current experimental evidence would predict no measurable exchange under physiological conditions. Thus it has been determined that nickel in planar complexes did not exchange with ionic nickel (11), and, in agreement with this generalization, it was found that Fe^{+++} in Fe-porphyrin showed no exchange at room temperature (12). The extension of this reasoning to cobalt is justifiable on theoretical grounds: it was found that CO^{++} fell between Ni^{++} and Fe^{++} in order of stability of its complexes (13); and Co^{+++} is bound in porphyrins with the same exceeding firmness as Ni^{++} (14).

The complexity of the B_{12} molecule makes its synthesis unlikely in the near future. Introduction of cobalt into the B_{12} molecule must therefore be accomplished by feeding radioactive cobalt to protists or animals [8] and then recovering B_{12} from the products of growth. If on chemical fractionation of the cells it should be found that radioactive cobalt occurs in fractions besides those which are B_{12}-active, this would indicate a further biological function for cobalt. Whether cobalt has a plurality of functions is an unanswered question.

Microbiological synthesis of B_{12} is now an industrial reality: B_{12} is produced in relatively high concentration by actinomycetes, and it is therefore a most welcome by-product of the production of streptomycin (15) and aureomycin (6). An investigation of the production of B_{12} by actinomycetes and by certain gram-negative bacteria (16) is interesting on other than practical grounds. It may delineate the metabolic pathway of synthesis of B_{12}. Organic compounds which specifically stimulate production of B_{12} in the presence of excess cobalt, if they exist, may be actual precursors of B_{12}, built into the molecule, as has been shown for phenylacetic acid (and derivatives) in penicillin production (17).

COBALT, B_{12}, AND THE GENE

The transitional elements in the periodic table show outstanding catalytic activity. The concept of "essentiality" of a catalytic element calls for scrutiny. The activity of B_{12} at exceedingly low concentrations makes it highly likely that B_{12} (and, necessarily, cobalt) has a catalytic role in cellular syntheses. For certain lactobacilli, B_{12} is replaceable by one or more purine or pyrimidine nucleosides occurring in desoxyribose nucleic acid (7), none of which contain B_{12}. As these nucleosides must be supplied in hundred-fold or higher amounts as compared to B_{12}, it is reasonable to suppose that B_{12} is concerned with the enzymatic synthesis of these nucleosides and not *vice versa*. Desoxyribose nucleic acid (DNA) which contains the active nucleosides is an important building block of genes; DNA is unquestionably intimately associated with the gene (18). The chain of synthesis $Co \rightarrow B_{12} \rightarrow$ nucleosides should logically be climaxed by DNA itself, and this has been reported: *Lactobacillus bifidus* requires either B_{12}, desoxyribose nucleosides, or DNA (19). Several other lactobacteria which require one or more desoxyribose nucleosides can also utilize DNA (20); replaceability of these factors by B_{12} is not mentioned. The sequence of synthesis may begin at a more complex stage: a strain of *L. bifidus* stud-

[8] The paucity of B_{12} in land plants lends a special interest to following the fate of radioactive cobalt in them. Radioactive cobalt may serve to direct attention to non-B_{12} cobalt-containing compounds having a function equivalent to that of B_{12}.

ied by Tomarelli *et al.* (21) did not utilize B_{12}, although desoxyribose nucleosides and DNA satisfied this portion of the growth requirement. The route of synthesis $B_{12} \rightarrow$ DNA outlines the pattern of desoxyribose-nucleic-acid-component growth factors for lactic bacteria, and suggests some hypotheses on the nature of genes and viruses.

Thus the trail of cobalt in the organism leads to the basis of cellular organization, the gene. Yet it is improper to conclude that cobalt is a part of all living organisms. If cobalt does not actually occur in the essential cell constituents whose syntheses it catalyses, organisms which can obtain these products preformed from other cells could bypass the cobalt requirement in their own protoplasm. This argument is expanded in the discussion of cobalt and B_{12} in viruses.

VIRUSES

Viruses may typify the physical minimum consonant with biological individuality. The simplicity of viruses, far from being primitive, may be the result of evolutionary adaptation of certain parasites to growth within the cells of the host. The parasite, once intimately in contact with the host, may opportunistically discard more and more of its own syntheses in favor of diverting increasingly elaborate molecules from the host into the parasite's own self-duplicating substance (22). The culmination of this evolution is a virus which derives from its host all but the final building blocks for its own irreducible complement of genes. It is not surprising, therefore, that viruses prove to be rich in nucleic acids and proteins (23). Heydén (24) remarks that "all the virus species investigated [by him] behave as parasites on the nucleoprotein-forming parts of the cell." These ideas are expressible in terms of growth factors: a virus needing large and reactive prefabricated molecules must obtain them from a living cell and not from a chemically degraded artificial culture medium.

The subject of growth factors which are thermolabile and of large molecular size is in its infancy, but already it changes our ideas of the permeability of cells to large polar molecules. The first growth factors to be identified were rather small molecules: thiamine, nicotinic acid, *p*-aminobenzoic acid, biotin, riboflavin, pantothenic acid, and pyridoxine. When di- or triphosphopyridine nucleotides (cozymase) were identified as growth factors for bacteria of the genus *Hemophilus*, permeability to these large metabolites was tacitly considered to be connected somehow with the extreme specialization of *Hemophilus* for parasitism. It was thought unlikely that free-living organisms would admit such large polar molecules. Intracellular parasites, because of their intimate association with host cells, are in most intimate contact with large and vital molecules and they have penetrated behind the molecular sieve represented by the cell membrane of the host. The complicated nutritional requirements of viruses finds its counterpart in the extreme difficulty of culturing nonviral intracellular parasites as compared with their extra-cellular relatives. A classical example of this is the human leprosy bacterium, which, unlike the related organisms causing tuberculosis and Johne's disease, has not been obtained in artificial culture despite numerous efforts. It was widely held therefore that free-living organisms could not use growth factors having high molecular weights while viruses and other intracellular parasites were enabled to use large molecules. Support for the view that even extracellular parasites may have complex requirements and may be able to absorb large molecules is provided by the extreme difficulty in identifying the growth factors for animal tissue cultures: unstable, high molecular weight materials appear to be needed (25).

Another instance of bacterial syntheses being modified by high molecular weight substances, externally supplied, is furnished by the *Pneumococcus* transformation factor. But here preparation of the surface of the bacterium by something in blood serum, perhaps of an enzymatic nature, is assumed to be necessary (26). It is not clear whether similar substances, derived from the bacterium rather than from serum, are required for the action of the transformation factors for *Escherichia coli* and *Shigella* (27).

The discovery that certain lactic bacteria require folic acid narrowed the gap between the size of growth factors known for free-living organisms as compared with those postulated for parasites. The molecular weight of the folic acid group of growth factors ranges from about 325 to a reported maximum for bacteriological activity of about 700 (28). Later it was found that certain lactobacilli required B_{12}, and could use desoxyribose nucleosides. Others could even use DNA (see above). The B_{12} requirement of algal flagellates is especially significant, in this connection, for their photosynthetic way of life emphasizes their extreme nutritional independence aside from their

growth factor requirement.[9] If there is a limit to the size of intact molecules which can be assimilated by free-living organisms, this limit must be revised sharply upwards in the light of the foregoing information. Correspondingly, the molecular limit of permeability for parasitic organisms in artificial media must be raised also.

In summary, it would appear that the metabolism of metals, when growth factors contain these metals, may be traced into very complex molecules. A systematic exploitation of the possibilities inherent in studies of the growth requirements of highly evolved parasites, particularly parasites with an intracellular stage, could make possible an even further analysis of function. A comparative study of the growth of various intracellular parasites of widely differing phylogenetic affinities, and dwelling in very different hosts, might throw into sharp relief some of the dynamic common denominators of living processes.

Difficult technical problems must be overcome before proceeding with the analysis of tobacco mosaic virus for components functioning as tools as contrasted with components responsible for the construction of final body substance. Tobacco mosaic virus is exceptionally favorable experimental material since it may be prepared with relative ease in bulk in a state of high purity. Growth factors are not likely to be present in viruses in concentrations permitting chemical or spectrographic analysis. Recourse must therefore be had to microbiological methods of assay, which have the further advantage of extreme specificity. The development of microbiological methods of assay is frequently arduous. Reliable assays of vitamins and amino acids by means of lactic bacteria were achieved only after rather large-scale researches; the investigators had to mark time on occasion while fundamental biochemical data accumulated. In fact, animal feeding tests are the sole methods available for analysis of B_{12} in most natural materials, for means have not yet been devised for liberating B_{12} in microbiologically active form from high molecular weight combinations. The use of microorganisms having an animal mode of nutrition—i.e., ingestion of solid particles—may provide a way out of this difficulty; the studies with the ciliate *Tetrahymena* show that it may be used in the future to determine the nutritional value of *intact* proteins (29) and of high molecular weight folic acid conjugates (30).

It would be most interesting to attempt the analysis of isolated chromosomes by microbiological methods and to explore the analogies between genes and viruses. For example, it would be worth investigating the extent to which methods of digesting chromosomes to liberate microbiologically active products applied to viruses. Another test of the concept that finished genic products may not permanently incorporate the tools that made them—the scaffolding—might consist of a comparison of the composition of cellular nuclei *vs.* cytoplasm with respect to the catalysts entering into the formation of DNA. Likewise, analyses for folic acid and *p*-aminobenzoic acid might serve as controls for the localization of B_{12}, for they too are concerned with the synthesis of nucleic acid, operating on a slightly different sub-assembly line, in this case for the fabrication of thymine and purines (31). Again, like B_{12}, folic acid and *p*-aminobenzoic acid have structures distinctly different from the products in the formation of which they are concerned. The replaceability of B_{12} by divers nucleosides and by DNA adds weight to this hypothesis. Folic acid is also reported replaceable by DNA for certain lactic bacteria (32). The localization of cobalt in the cell—a logical extension of this direction of inquiry—might profitably be studied with the aid of radioactive cobalt, by the autoradiograph technique. Microbiological assay might provide an ultra-sensitive method for detection of cobalt. This will be feasible when microbiological deficiencies for cobalt have been achieved in the laboratory.

IRON, HEME PIGMENTS, AND HEMOFLAGELLATES

A problem paralleling that of cobalt-B_{12}, but likely to be technically easier in some ways, is that of iron and hemin. All organisms utilizing molecular oxygen employ heme enzymes for this purpose. Hemoglobin, although not ordinarily classified as an enzyme, is structurally akin to the heme enzymes (for critical reviews see (14, 33)). These enzymes contain the iron porphyrin pigment hemin as a prosthetic group. Hemin contains iron bound in a planar ring complex. The same questions come to mind as with cobalt-B_{12}: does the iron in hemin account for all the iron requirement? There are at least three distinct ways of attacking the problem: (1) by studying the iron requirement of organisms requiring hemin as a growth

[9] Unpublished experiments indicate that *Euglena gracilis* grows rapidly in light in synthetic media with CO_2 as sole carbon source aside from a trace of B_{12} and thiamine. Hence it photosynthesizes normally.

factor, (2) by studying the iron requirement of organisms (all anaerobes) *not* containing hemes, and (3) by studying the iron requirement of organisms containing hemes but capable of growth under anaerobic conditions, where there is no evidence at present that the well-defined heme enzymes have an essential function.

THE HEMOFLAGELLATES

A number of parasitic protists require hemin as a growth factor. Unfortunately these have complex growth requirements in other respects. Those members of the bacterial genus *Hemophilus* which require hemin have, by all evidence, complicated growth requirements. Most of these requirements await identification and at least one is thermolabile—a decided practical inconvenience. At our present state of knowledge they are obviously unsuited for experimental studies of the iron requirement. Certain members of the group of protozoan flagellate parasites known as the hemoflagellates (which include the trypanosomes causing African sleeping sickness) are more tractable: they can be grown in autoclaved media (34). A detailed investigation of a hemoflogellate, *Herpetomonas culicidarum,* which dwells in the lumen of the gut of mosquitoes, has revealed that aside from an unidentified growth factor, the nutritional requirements are rather simple, and the unknown growth factor is readily obtainable in concentrated form (Weber, M. M., and S. H. Hutner, unpublished). Their iron requirement is almost certainly much greater than the cobalt requirement. Lwoff (35) calculated that a related hemoflagellate required about 746,000 molecules of hemin for each cell division. Each molecule of hemin contains one atom of iron. Assuming that the medium can be rendered virtually iron-free, the same problem arises as with B_{12}: that of ensuring that the bound iron supplied in the hemin molecule is not freed. It is known that iron in heme does not exchange with iron in the surrounding medium under mild conditions (12). A detailed study of the binding of iron in hemin, investigated with radioactive iron, at the temperatures encountered in sterilization by steam, and at all the pH's likely to be encountered in culture media, would assist the proper interpretation of iron-hemin experiments.

A hemin-requiring organism such as *H. culicidarum* may be used as a reagent for hemin. This offers a uniquely sensitive biological test for hemin to supplement less specific spectroscopic methods, and may make possible the experimental answer to certain pertinent questions. Methods for extracting hemin in the free condition from high molecular weight combinations are fairly well worked out.

Does tobacco mosaic virus contain heme pigments? A highly evolved virus would not be expected to contain hemes or to need much energy for syntheses, particularly for oxidative assimilations with molecular O_2 as hydrogen acceptor, for utilization of oxygen would entail possession of a fairly complicated sequence of heme enzymes, unlikely in a virus. Instead, one would expect to find the end products of syntheses, together with the utilization by the organism of only the most concentrated forms of chemical energy. It would be in keeping with our current concept of a virus if it utilized what appears to be the ultimate source of energy for cellular syntheses—high energy phosphate. Perhaps failure to provide a versatile energy source such as adenosine pyrophosphate may be at the root of the complete lack of success encountered in culturing viruses in artificial media. If viruses require so unstable an energy source as acetylphosphate, the outlook for artificial culture is gloomy indeed. Intimations of the reliance of viruses and other highly specialized parasites on unstable phosphorylated metabolites are furnished by certain gonococci which require cocarboxylase (thiamine pyrophosphate) (36), and by certain lactobacilli which use pyridoxamine phosphate under circumstances where pyridoxal is inert (37). *Bacterium tularense,* an intracellular parasite readily obtained in artificial culture, requires a preformed phosphoryl group in compounds such as cocarboxylase, adenylic acid, and adenosine diphosphate, but the best growth is obtained with adenosine triphosphate (38). Progress in tissue culture and in the culture of viruses in chemically defined media should accelerate as the requirements of less specialized, more readily culturable parasites become known.

Certain metals are known to be essential to the cell, but their physiological role is obscure. Among such metals are molybdenum, zinc, and manganese. The presence of zinc or manganese is known to be necessary for the functioning of certain enzymes, but the metals may be present in ionic form—not combined in a definite compound. The meagerness of our information here is well illustrated by the case of magnesium. Magnesium is necessary for the functioning of a number of enzymes concerned in glycolysis. The appearance of magnesium as part of the chlorophyll molecule

hints at more general functions which depend on the binding of magnesium into well-defined molecules.

The locus of action of copper in the cell is poorly understood because of another sort of difficulty. Copper appears to be part of large protein molecules having a respiratory function. It has not been found as part of a smaller prosthetic group in the sense that iron is part of the hemin molecule, which in turn is part of the large hemoglobin molecule (molecular weight 64,000). If growth factors could be found in which these metals were present as definite constituents, a means would be available for getting at the localization in the cell of the functions of these metals. The circumstance that *H. culicidarum* requires an unidentified growth factor is therefore not unwelcome, although it complicates the removal of iron from the medium.

Another attractive reason for inquiring into microbial growth factors is the possibility of linking them with vitamins for animals. Much of the remarkable progress in the investigation of water-soluble vitamins has depended on the exploitation of microbial assay methods to elucidate the properties of the vitamins. As mentioned earlier, there are scattered indications that poultry and certain mammals have a vitamin requirement not satisfied by B_{12} together with the other known factors. Any microorganism with an unidentified growth factor requirement is a potential reagent for the assay of one or another of the unidentified animal vitamins. The supply of microorganisms with clearly defined new and unidentified [10] growth factors is scanty at present. A frontal attack on the nutrition of the leishmanias and the trypanosomes, with their unknown thermolabile factors, might well be deferred until the nutritionally simpler herpetomonads are worked out, with concomitant development of an improved basal medium for investigation of the trypanosomes and leishmanias. There is much evidence that the metabolism of the herpetomonads definitely resembles that of their trypanosome and leishmania relatives.

Once a growth factor is identified chemically, it provides a rational basis for chemotherapy, because it then becomes possible to design highly specific anti-metabolites intended to damage the parasite by interference with the utilization of growth factor in question. As certain important pathogens among hemoflagellates are refractory towards the currently available drugs, this represents another practical motive for nutritional investigation of the group. The analysis of the mode of action of the chemotherapeutic agent helps in turn to uncover metabolic relationships of the growth factor itself. An excellent recent example of this is the finding that a desoxyribose nucleoside, thymidine, overcame the toxic action of methylfolic acid, an antagonist of folic acid (39). Since human pernicious anemia involves defects in the utilization of both folic acid and B_{12}, this suggested trial of thymidine as a replacement for B_{12} in microorganisms. Thymidine proved to take the place of B_{12} for several organisms, and this in turn suggested trial of other desoxynucleosides, some of which proved active. This is today an exceedingly active field of research, and demonstrates in unmistakable fashion the interweaving of theory and practice so characteristic of the growth factor field.

But broader issues appear. As an array of parasites, the hemoflagellates are unsurpassed in wealth of available culturable species. Their adaptation to parasitism has kept pace with the evolution of their hosts. Some hemoflagellates are simple in morphology and most of them are apparently not capable of infecting vertebrates. They include dwellers in the latex of lactiferous plants which are transmitted from plant to plant by hemipterous insects (an extraordinary variation on the theme of the cycle of infection: vertebrate blood to arthropod gut to vertebrate blood). Some are blood parasites on lower aquatic vertebrates which have leeches as vectors. The group further includes trypanosomes of birds and mammals. In some of them the insect vector has been entirely lost. One of them is transmitted venereally (*Trypanosoma equiperdum*). The leishmanias are adapted to an intracellular parasitic life, but in culture or in the insect host they revert to herpetomonad-like forms indistinguishable from ordinary insect gut parasites. These leishmanias, along with *Trypanosome cruzi* (which has a curious combination of leishmania and trypanosome characteristics), should be of signal value in understanding the processes which in other lines may have led to the evolution of viruses. One may even speculate that leishmanias were the ancestors of the virus-like piroplasms.

[10] The somewhat paradoxical expression "new but unidentified" refers to the fact that the growth factor is clearly different from any metabolite of known nutritional significance. This implies that media are available for demonstrating a quantitative, unequivocal response to the factor, and that the properties of the factor differ from those of unidentified factors described for other organisms.

Many members of the genera *Leishmania* and *Trypanosoma* have been grown in artificial media containing unheated blood. The study of the nutrition of the hemoflagellate stock (marshalled in Wenyon's (40) magnificent treatise) might provide the means of identifying more and more of the dynamic constituents of the animal cell in the guise of growth factors for hemoflagellates. As already indicated, the pattern of increasing complexity of nutritional requirements in the hemoflagellate stock may parallel the course followed by the parasites that became animal viruses. The multiplicity of growth factors remaining to be identified in the hemoflagellates challenges the best efforts of vitamin enthusiasts.

ANAEROBIC ORGANISMS

If an organism does not utilize molecular oxygen for respiration, it may be able to dispense with all the functions for which heme enzymes are required. The clostridia, classical obligate anaerobes, do not have spectroscopically observable heme (41).[11] An investigation of a possible iron requirement of clostridia would indicate whether iron has functions apart from the construction of heme enzymes. The practicability of this line of experimentation is enhanced by the availability of clostridia with simple growth requirements, such as *Cl. kluyveri,* which can be grown with acetate and ethanol as substrates (42). These substrates lend themselves to rigorous purification. The butyric acid fermentation of clostridia is inhibited by carbon monoxide, and this inhibition is reversed by light. From the action spectrum of this reaction, Warburg (43) concluded that a non-hemin iron compound was involved. Without direct characterization of the enzyme, the nature of the metal, if any, concerned in the butyric fermentation, must remain a moot question. Heme compounds probably account for most, if not all, of the iron used by aerobic cells. It is quite possible that anaerobic cells may require very minute concentrations of iron, and difficulties may arise in obtaining media sufficiently free of iron for critical work. It has been reported that one Clostridium at least (*Cl. welchii*) makes no growth without iron (47). The technique developed for studies of growth factors under anaerobic conditions (44) might prove helpful in attacking this problem.

The third approach—the study of the iron requirement of heme-containing organisms that may be grown anaerobically—has received very limited attention. Waring and Werkman (48) grew the facultative anaerobe *Aerobacter indologenes* in media very low in iron. The resulting cells were virtually devoid of heme enzymes, formic dehydrogenase, and hydrogenlyase, and growth was reduced to 3 per cent of normal. These authors suggest that the production of carbon dioxide and hydrogen from formate by bacteria is due to a carrier, perhaps a heme enzyme, linking the dehydrogenation of formic acid and the hydrogenase reaction $H_2 \rightleftharpoons 2H \rightleftharpoons 2H + 2\ e$. If the enzymes activating molecular hydrogen contain iron, the problem of the universality of the iron requirement is transformed into the problem of the universality and essentiality of hydrogenase in anaerobes. The finding that a strain of *Hemophilus influenzae* still needed hemin when grown anaerobically (14) suggests that hemin has a function other than that of the utilization of molecular oxygen.

These evidences of a multiplicity of anaerobic functions for iron may imply non-heme essential functions for iron in aerobes as well.

TECHNICAL CONSIDERATIONS

It would be difficult to find a branch of science whose progress is more immediately dependent on the development of both broad strategies and the minutiae of technique than the subject of the nutritional role of trace elements. In the past this frontier of investigation was shunned by all but the most patient of workers, for the methods then in use for induction of inorganic deficiencies were laborious in the extreme, and the results were hardly commensurate with the effort. The data on the trace element requirements of fungi, the protists which have received the greatest attention in this respect, are summarized by Foster (17). Advances in physics, chemistry, and technology have brought forth powerful new methods of attacking the problem.

CHELATING AGENTS

In physical and chemical experimentation the situation frequently arises where the effect sought, or the quantity to be determined, differs so little in character or magnitude from random background effects introduced by the environment, that attempts to isolate it by simple reduction of this random background are hopeless. In cases of this sort special means must be devised to throw the variables which are being investigated into sharp relief. This difficulty attends studies of trace ele-

[11] The statement that certain clostridia contain catalase (43) has not been supported (44).

ments, and is particularly serious for the so-called transitional elements. Unless special methods are used, omnipresent dust and random contaminations from containers, water, and chemicals may blur into uselessness attempts to induce clear-cut deficiencies. A simple method of achieving this contrast is to add to the culture media organic compounds forming complexes with metals. The metal ions bound in such complexes are relatively unavailable to the organism. Thus a marked deficiency is set up. Additional metal ions must be added to the culture medium to compensate for the removal of the metal from the field of action by the complex-former. If the metal thus added is extremely pure, the background contamination can be so reduced as to become negligible in comparison with the concentration of metal which must be experimentally added to overcome the deficiency. Using this procedure it has been found that the requirement of microorganisms for metals such as manganese, zinc, calcium, and magnesium can be established quantitatively with little difficulty, using chemicals of ordinary reagent grade.

The use of such chelating agents appears indispensable in studies of trace element requirements. It is also essential in obtaining good growth of organisms having relatively simple growth requirements. A detailed exposition of the considerations governing the use of chelating agents therefore seems justified.

In complex natural media (i.e., those containing bacteriological peptones, yeast or liver extracts, meet infusions, and the like), metal ions are not ordinarily limiting factors for growth unless growth is heavy: natural materials, especially if originating from cellular sources, are rich in essential metals. Despite the presence of concurrent high concentrations of dissolved metal and phosphate, the metals may not precipitate as phosphates or basic hydroxides even if the media are neutral or alkaline, since natural materials contain solubilizing chelating agents which reduce the concentration of free metal ions below the point of precipitation. Many biological compounds act as chelating agents: α-amino acids, o-hydroxy carboxylic acids, glycerol etc. (For a discussion of the structural prerequisites for chelation see (49).)

Thus, most natural media amply satisfy the metal requirements of microorganisms. Indeed, such requirements can be satisfied in solutions containing exceedingly low concentrations of available metal ions, where by far the greater portion of the total metal is bound in biologically unavailable complexes. This seems reasonable if the surface of the organism is assumed to be provided with constantly renewed chelating groups capable of competing for metals with the chelating compounds in the surrounding medium.

Chelation thus appears to be a fundamental biological mechanism for the mobilization of metals, and chelating agents are widely distributed in living things. Blood contains citrate, an exceptionally effective solubilizing chelating agent[12] on which depends the transport of calcium to and from bone (50). Less is known about the mechanism of metal transport in plants. It is difficult to understand, however, how plants could exist on a netural or alkaline soil low in organic material unless the root hairs excreted a metal-solubilizing substance such as citric or malic acid. Land plants are unusually rich in such acids.[13]

If one attempts to grow organisms in synthetic media—i.e., media composed of chemically defined components—the availability of metals may become an acutely limiting factor. When metals are present in concentrations below those inducing precipitation, they may be sufficient to support only a limited growth. When metals are supplied (in the absence of a complex-former) in the amounts needed for heavy growth, they may be precipitated or prove toxic. These difficulties may be circumvented by adding a solubilizing chelating agent to the medium. This however introduces further complications. By thus lowering the effective background contamination of trace metals to the point where it no longer suffices for growth, the range of substances which must be added experimentally in order for growth to be achieved is extended well beyond the metals known to be essential for life. The added substances must now include elements the essentiality of which cannot be quantitatively demonstrated. These elements may be non-metallic in nature quite as well

[12] Many of the familiar chelating agents form insoluble compounds with certain metals, e.g., oxalate with calcium, and dimethylglyoxime with nickel. If the chelating molecule possesses an excess of hydrophilic groups while the metal-binding configuration remains unchanged, the metal may be bound just as tightly, but the complex as a whole will be soluble.

[13] Certain carboxylic acids such as malic acid are an integral part of the Krebs tricarboxylic acid cycle. As this chain of reaction is in part the mechanism by which the building blocks of proteins, carbohydrates, and fats are interconvertible, and rendered available either for energy-yielding or synthetic purposes, acids such as malic and citric (or acids closely related to citric) are fundamental constituents of living matter.

as metallic. Iodine and boron are familiar examples of elements which have been demonstrated to be essential for growth in higher animals and in flowering plants respectively, but the essentiality of which for microorganisms has not yet been established (as was indeed also the case for cobalt). It seems clear to us from many experiments that synthetic media which are to promote consistent and heavy growth of microorganisms must include complex-formers as well as a rather wide range of trace metals in balanced proportions. Some of the latter can be demonstrated to be definitely essential for living processes. Others are still in what might be called the twilight-zone of activity. Certain of these media are given later in this paper.

Calcium Requirements

The demonstration of calcium requirements for living cells has become a simple matter, and serves to illustrate the application of chelation technique. The method used to arrive at a quantitative estimation of the calcium requirement is to plot the increasing concentrations of calcium required for growth as the concentration of citrate in the medium is increased. By extrapolating this curve to zero values for citrate concentration, the intercept of the growth-response curve with the calcium axis affords a measure of the absolute calcium requirement. For the marine diatom *Nitzschia closterium,* the calcium requirement for moderate growth is about 0.5 mg. per cent—an amount readily supplied as an impurity from chemicals or glassware. The same procedure has been applied in investigating the calcium requirement of *Chlorella, Euglena,* and the water mold *Achlya* (Reischer, Helen S., unpublished). Since many of the systems concerned in the distribution of energy in the cell are considered to be activated by calcium, such as adenosinetriphosphatase (51) and other enzymes (52), proven absence of a calcium requirement in these organisms would have implied a highly improbable condition—namely, that they possessed a different mechanism of energy exchange.

Several kinds of evidence indicated that apparent calcium requirements could be satisfied only by calcium. Mixtures of rare earth elements, used at nontoxic levels, were ineffective. Purification of calcium through the oxalate did not alter its activity. The addition of comparable amounts of other elements having a greater affinity for chelating agents, e.g., manganese, zinc, and cobalt (13), did not significantly affect the calcium requirement.

The fact that an ever-widening range of elements can be shown to be essential or nearly essential for growth as the medium is pushed to lower and lower concentrations of essential constituents, as their significant impurities are deliberately supplied, serves as the starting point for a wide range of possible experiments involving a more and more rigorous control of the experimental variables. A few of the technical prerequisites for such investigations may be touched on.

Ethylenediamine Tetraacetic Acid as a Chelating Agent

The effectiveness of the chelation technique for defining trace metal requirements is determined by the choice of chelating agent ("ligand"). The criteria for a "good" chelator may be listed as follows:

1. The compound should form very stable complexes. Therefore it should be effective at low molar concentrations to minimize the risk of exceeding the osmotic tolerance of the organism.

2. It should possess a large net charge, conferred by bulky hydrophilic groups. The complex-former, whether acidic or basic, should be predominantly in the ionic state at physiological pH's and hence likely to be sufficiently soluble there. A large molecule with evenly balanced polar groups tends to be only slightly soluble at the isoelectric point. To aid in the interpretation of results, it is preferable that the molecule be bulky enough, because of multiple hydrophilic groups, so that the organism will be impermeable to the complex-former. Thus essentially all the metal-binding will take place in the external medium and not within the cell. These specifications rule out for this use many of the reagents employed in analytical chemistry for the separation and estimation of metals, such as 8-hydroxyquinoline.

3. The compound should be colorless and photostable. It should also be thermostable, at least to sterilizing temperatures.

4. The compound should have free rotation of the metal-binding chelate of the complex-former. This allows inclusion into strain-free rings of metals over the widest possible range of ionic radii. The influence of restrictive steric effects in metal-binding should be minimized, especially in exploratory work. The geometric rigidity introduced by double bonds in the complexing molecule introduces an irregularity in the firmness of binding of metals which is as yet unpredictable (53). It may be desirable, in special investigations, to use

chelating agents which are highly specific for certain metals. Such agents are known to be characterized by rigid molecular frameworks into which, because of spatial limitations, only the specific metal can find ready entry. A good example of this is presented by the structure of the porphyrin ring. Use of chelating agents of this type—perhaps one of the soluble phthalocyanine dyes—might provide the means for studying the need for metal ions of smaller dimensions. The fact that the cell makes use of compounds such as porphyrins (B_{12} may well turn out to be a porphyrin-like compound) is a hint of how the cell may achieve narrow specificities in its functional use of metal ions.

5. The compound should not be capable of being metabolized and should not be toxic for reasons unconnected with its metal-binding properties. If the organism metabolizes the complex-former there will be an apparent lowering of the metal requirement. In investigations not centered around the role of metals or questions of energy metabolism it is preferable, for the sake of obtaining heavy growth, to have the complexing agent serve as an auxiliary substrate. This has the further advantage of keeping the osmotic pressure low. Furthermore, as the organisms proliferate and absorb an appreciable proportion of the total metal ions in the medium, the decline in the concentration of metal ion so produced is compensated by the liberation of free metal ion as the complexer is consumed. Malate, a good substrate as well as a fairly effective metal-binder, has been especially useful in work with purple bacteria (54), and with *Euglena gracilis* (Hutner, S. H., and T. Cayle, unpublished). Similarly, glycine, employed both as a buffer and as a supplementary metal binder, is facilitating culture work with green algae.

Ethylenediamine tetraacetic acid ("EDTA"; fig. 2)[14] admirably meets the specifications for a non-metabolizable solubilizing complex-former (55). Its complexes with metals have been subjected to detailed chemical investigation (56). It has lately been shown that in the absence of major spatial limitations in the structure of the molecule, the order of intensity of chelation of metals is independent of the nature of the chelating agent (13). This implies that EDTA will function in the same manner as other chelating agents.

[14] Listed in *Chemical Abstracts* as ethylene-*bis* (iminodiacetic) acid.

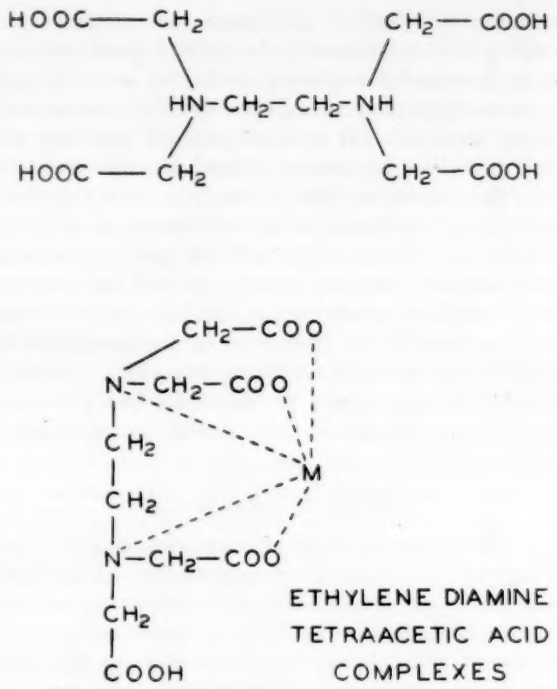

FIG. 2. Complex formation by ethylene diamine tetraacetic acid (after Schwarzenbach).

Purification of Major Inorganic Nutrients

1. *General remarks*

Heavy growth has been obtained in simple EDTA-containing media, but this has been largely an empirical achievement. The reproducibility of this work can be confirmed only if the necessity of each ingredient is established in quantitative terms. It is necessary that each component of the medium be subjected to rigorous purification, by a variety of independent methods. If the requirement for the purified component is then found to be quantitatively unchanged, it will be safe to assume a genuine necessity for this component, and the proper concentration for its use can be determined. When every component has been tested in this way, it should be possible to construct a medium in which every essential requirement is adequately fulfilled but is not in excess. At present this goal is far in the distance. Foster (17) in his treatment of the historical development of knowledge of trace metals has described how the older plant physiologists attempted to achieve maximal growth in the most economical manner by using just sufficient amounts of the inorganic nutrients recognized at that time: potassium, nitrogen, phosphate, magnesium, calcium, and sulfur. At first it

seemed that these efforts would be crowned with success. But as chemicals of higher purity became available, growth in such experiments became poorer and poorer. The newer experiments failed where the older ones succeeded because the new chemicals lacked essential elements. As more and more trace elements are identified, smaller amounts of the major constituents of media suffice for good growth. This is because such constituents need no longer serve as carriers for contaminating trace elements.

The following may be considered a rational experimental procedure for obtaining a good synthetic culture medium:

(1) A quantitative growth curve is constructed for each component of the culture medium.

(2) A new medium is made up in accordance with data from these curves, by using concentrations of each element known to be just sufficient, or with a calculated excess, for maximal growth.

(3) The determination of the growth curves is repeated, by again varying constituents of the medium one at a time.

One might expect this procedure to lead to a medium in which the major ingredients are decreased in amount, and the concentrations of the trace elements relatively increased. It has been found experimentally that as this type of analysis is continued, growth gradually becomes scantier and scantier instead of increasing. The most likely explanation of this unexpected result is that as the media become more dilute and at the same time more closely coordinated with the needs of the organism, a progressive loss of unidentified trace elements leads to a rather erratic decline in growth. Attempts to restore growth by the addition of one element at a time have been unsuccessful. The working hypothesis to account for this result, devised to guide subsequent work, has been that several unidentified elements are needed in combination and that growth curves are only reliable when they fulfil the criterion mentioned above —that they remain unchanged no matter how far the nutrient is refined. These conditions have been met for only a few of the nutrients. Thus an enormous amount of work lies ahead, which will stress a critical examination of purification procedures.

Recourse to analysis of ash has not been had for several reasons. First, as noted by many workers, the selectivity of cells for inorganic nutrients is, on the whole, limited, despite a number of remarkable exceptions, such as the selective accumulation of potassium. Thus there is likely to be an accumulation of excessive amounts of both essential and nonessential elements, if these are present in excess in the environment ("luxury consumption") and these will be represented in a nondescript assortment in the ash. Second, in the course of either wet- or dry-ashing, volatile elements are likely to be lost, and many elements, particularly those in the central families of the periodic table, become insoluble. This is true even of certain transitional elements if an excess of phosphate is present: exceedingly insoluble pyrophosphates may be formed. Further, the very reagents used for wet-ashing, or for dissolving ash, may themselves be the bearers of significant amounts of contamination. It is to be noted, however, that the use of ash in the culture medium may provide a convenient method of supplying a mixture of unidentified essential elements otherwise difficult to assemble. The components responsible for the effectiveness of such ash in restoring growth may then be identified by conventional chemical analysis.

So far in the purification procedure, recourse has been had largely to procedures involving fractional distillation and partition between solvents; these have been shown to be efficient methods (57). Some pitfalls in recrystallization (the method used for much of the older work) are discussed by Sandell (58).

2. *Purification of major nutrients*

Nitrogen. The problem of supplying nitrogen in adequately pure form has proven comparatively simple. Nitric acid is distilled through a vacuum-jacketed, glass-helix-packed column so as to obtain a fraction boiling through a narrow range, with a minimum of entrainment. The acid distillate is then neutralized with ammonia vapor. The toxicity associated with ammonia in culture media at neutral and slightly alkaline pH has been found, in agreement with the work of Fries (59), to be largely attributable to the complexing properties of the ammonium ion and to a lesser extent to competition with potassium, in the manner described for lactobacilli (60). Hence it may be overcome by the judicious addition of trace metals and additional potassium. Ammonium ion, therefore, acts as an auxiliary (and a very pure) metal-binding agent of the Werner type.

Phosphate. All commercial phosphate is obtained from phosphate rock, a deposit derived from

the remains of marine organisms. Such rock is actually a solidified biological enrichment culture. Phosphate has the property of forming complex (heteropoly) acids with many elements. Hence it is likely to be an efficient carrier of essential trace elements. The method of purification has been to start by distilling phosphorus oxychloride, a liquid boiling at 107° C., and then to add water to the distillate, forming phosphoric and hydrochloric acids according to the equation $POCl_3 + 3H_2O \rightarrow H_3PO_4 + 3HCl$. The hydrochloric acid is distilled off, and the phosphoric acid is neutralized either with ammonia or with potassium bicarbonate.

Potassium. Potassium bicarbonate was treated by precipitating it from aqueous solution by the addition of redistilled isopropanol. Other methods which could be employed alternatively might exploit the solubility of the hydroxide in ethanol—a property seemingly shared only by the alkali metals above potassium in the periodic table. Metallic potassium might be a good source of the purified element.

Magnesium sulfate. Purified magnesium metal from a commercial source was treated with H_2SO_4 prepared by distilling sulfur trioxide fumes into distilled water. The neutral product was recrystallized from water.

Calcium. Calcium is a major requirement of land plants, and preliminary results with a marine diatom (61) point to the possibility that marine algae may have a comparably high requirement. Calcium salts, like phosphate, are derived from marine sediments of biological origin. Calcium is likely, therefore, to be a major source of contaminating trace elements, and its purification is difficult. Experiments now under way in commercial laboratories on the growth of calcite crystals may make available a relatively pure source of calcium. The purification of calcium metal by means of solubility in liquid ammonia offers interesting possibilities (62).

PURIFICATION OF TRACE ELEMENTS

The biological significance of cobalt contamination in spectroscopic-grade iron (p. 153) illustrates the danger inherent in the assumption that because an element is needed in relatively small amounts, its purity may not be as important as that of a major ingredient. Work in refining such components has not progressed to the point where a systematic treatment can be presented. Some guiding principles, however, can be listed.

1. *Use of Chelating Agents in Removing Trace Metals*

This subject is adequately discussed by Foster (17) and will only be briefly mentioned here. Certain chelating agents, notably 8-hydroxyquinoline and dithizone, form metal complexes which are soluble in nonpolar solvents such as chloroform or ethyl acetate. The trace metals are made to pass into the solvent phase. This is conveniently done with a continuous-extraction apparatus. Most transitional elements are especially well removed from major constituents by this type of procedure, which has been extensively used in analytical chemistry. The full benefit of this purification can be realized only if the trace elements removed are replaced, not by ordinary grades of trace elements, but by highly purified components—a thing not yet done.

2. *Iron*

This metal well illustrates the variety of procedures which can be used. The starting point for purification may be the metal itself, purified either by distillation or by electrolysis. The metal may be dissolved in aqua regia and then purified further. Another suitable starting material is sublimed ferric chloride, which is commercially available. While many metals form volatile chlorides, few volatilize in the same temperature range as ferric chloride. For some experiments with purified media, ferric chloride was dissolved in 6 *N* HCl, and continuously extracted from the aqueous phase with isopropyl ether, a procedure which leaves nearly all other metals behind (63). The isopropyl ether was removed by heating, and the residual ferric chloride was dissolved in dilute HCl, itself readily obtained in highly pure form by distillation.

Iron forms a liquid carbonyl boiling at 102° C. Its thermal decomposition yields the free metal which is exceptionally pure, as few metals form volatile carbonyls and few of these carbonyls themselves boil in the same range. Metallic iron so prepared is commercially available.

The metal carbonyls illustrate the unexpectedness of sources of contamination in this type of work. Baudisch (64) reported that when crude sulfur was employed as a substrate for the sulfur-oxidizing *Thiobacillus,* it supported growth without the addition of trace metals. Distilled sulfur supported growth, but not sulfur crystallized from carbon disulfide—a procedure which removes carbonyls. Addition of metals then permitted growth.

Commercial tank gases are commonly contaminated with metal carbonyls.

It can be seen from the foregoing that several simple alternate chemical methods, new to biology, are available for accomplishing the quantitative separation of iron from very similar elements such as cobalt and manganese.

3. *Sources of Trace Elements*

In addition to the starting materials mentioned above, initially purified free elements and compounds, including many of the rarer elements, are available from a commercial source,[15] thus providing a convenient starting point for further purifications.

EXPERIMENTAL ORGANISMS AND GENERAL TECHNIQUE

Choice of Organisms

A desirable experimental organism should have several properties. It should have simple organic requirements and should be capable of heavy growth in a limited volume of culture medium. Its growth should not be limited by the accumulation of toxic metabolic products. It should use substrates that lend themselves to rigorous purification. In practice, this rules out anaerobic, i.e., fermentative organisms except for special studies, because they form acids or other products of incomplete oxidation which eventually check growth at rather low levels. Since oxidation reactions which go to completion with molecular oxygen as the hydrogen acceptor are at least twenty times more efficient than anaerobic processes such as glycolysis, there is also likely to be much heavier growth from a given quantity of substrate with aerobic than with anaerobic organisms. An ordinary flask culture is analogous to a non-placentate embryo deriving its energy and the building blocks of its body-substance from a fixed and limited supply of yolk. Thus, such a culture must derive all its subsistence for growth and metabolism from its initial supply of nutrients, and the limit to the total concentration of nutrients that can be furnished at one time, once metals are removed as limiting factors, is frequently imposed by osmotic tolerances. The substrate may frequently make the largest single contribution to the osmotic pressure. For all these reasons, aerobic organisms are much more suitable experimental materials than anaerobic ones.

In vigorous aerobic growth in a stagnant (i.e., a non-aerated, non-agitated) culture, the limiting factor for growth becomes the availability of oxygen. It is inconvenient to supply oxygen mechanically in the procedures which we have used; unplugged, beaker-capped flanks for experimental cultures—a simple means of avoiding contact with biological materials such as cotton, and also for facilitating inoculation—would be impossible to use with mechanically agitated cultures, and forced aeration involves a rather cumbersome technique to ensure sterility of the air. One practicable method would be to study photosynthetic organisms, which either are self-oxygenating, like algae, or which assimilate substrates very efficiently with the aid of light, as do the purple bacteria. Another useful class of organisms for such experiments include those which can utilize nitrate as a hydrogen acceptor. As mentioned earlier, exceedingly pure ammonium nitrate is readily prepared by distillation. The denitrifying bacterium *Pseudomonas aeruginosa* tolerates high concentrations (greater than 4 per cent) of ammonium nitrate, and, in the presence of a good substrate such as glycerol, grows very vigorously, and the abundant nitrogen gas (and possibly also nitrous oxide) evolved also keeps the cultures well agitated.

A suitable seed plant, chosen to represent the crop plants, would be a most valuable addition to the roster of experimental organisms. It must be small and easily manipulated. Such a plant is the duckweed *Spirodela oligorhiza*. It may be maintained aseptically on agar slants and transferred with a loop in the conventional manner for handling microorganisms. It has the advantage over duckweeds previously used of having a higher temperature optimum for growth (approximately 34° C.). This allows employment of high levels of illumination—necessary for rapid dense growth—without risk of overheating. Ability to grow at summer temperatures has obvious practical advantages. Our strain of *Spirodela oligorhiza*, when supplied with a good inorganic basal solution and purified substrates, is not stimulated by natural materials in the manner reported by Gorham (65) for *S. polyrrhiza*.

Organisms that can grow on distillable substrates are much to be preferred. This is true even of photosynthetic organisms. Under the conditions of intensified proliferation possible in the laboratory, growth with carbon dioxide as a

[15] Johnson, Matthey & Co., Ltd. 1947. Catalogue of standardized substances for spectrography, chemical analysis, and research. London.

principal carbon source, even when the partial pressure of carbon dioxide is increased, is far inferior to that permitted by good organic substrates. Among the most useful substrates are acetic and butyric acids, succinic acid (prepared either from distilled succinic anhydride or from succinimide), and glycerol. At times combinations of substrates have been very successful. Media made with these free acids are conveniently neutralized with ammonia vapor.

Other desirable attributes of experimental organisms may be mentioned. A motile form is to be preferred, since motility is an excellent index of vigor. Certain motile chlamydomonads, e.g., a strain of *Chlamydomonas moewusii* Gerloff studied by Lewin (66), grow very rapidly at temperatures at least up to 35° C. on simple acetate media. This species is therefore to be preferred for nutritional investigations over the time-honored strains of *Chlorella*, a non-motile organism, with an optimum temperature of about 26° C., and preferring glucose (itself difficult to purify) as substrate.

Certain intensely aerobic organisms are capable of reaching exceedingly heavy densities in liquid media because of their habit of growing initially as a pellicle on the surface of the medium. Such growth is followed by the sinking of the pellicle, and the subsequent formation of a new pellicle. Such growth is characteristic of many acid-fast bacteria, including the tuberculosis organism, and of certain actinomycetes important as sources of antibiotics. These organisms grow well on glycerol as the sole substrate. As they are gram-positive bacteria, which have been shown to differ strikingly in metal requirements from gram-negatives (of which *Pseudomonas* is one), they merit special investigation.

Changes in the culture medium coincident with growth may result in large secondary differences in metal requirements. This may account for some of the extraordinarily large variations in the metal requirements reported for closely related organisms. If the substrate used is a chelating one (glycerol, glycine, malic, and anthranilic acid, etc.), it will initially be inhibitory to growth unless the concentrations of metal ions are increased to compensate for those bound by the medium. Once growth has started, consumption of the complex-former releases the metals. The problem therefore is that of getting the organisms over an initial barrier to growth; an insufficient appreciation of this situation has created some confusion in the literature (67).

The products of metabolism may include complex-formers such as hydroxy carboxylic acids and amino acids. In such case, a medium initially favorable may become very poor as the supply of available metal ions becomes progressively reduced. This condition appears in the culture of many acid-producing molds. It is thus probably more than a coincidence that the adjustments in metal ions for media containing citrate or EDTA, or high in glycerol, bear a considerable resemblance to those empirically developed for fungi.

Exaggeration of metal requirements is also obtainable by alkalinization of the medium, since up to a point the tightness of metal-binding increases with increasing pH. The reduction of nitrate by *Pseudomonas aeruginosa* leads to very alkaline media ($>$ pH 9.0). The consumption of an acidic substrate leads to the same result. It is an advantage to have the culture medium initially neutral as this lessens chemical attack on the glass and in addition allows more leeway before growth reaches the alkaline limit.

It is an advantage if the organism forms uniform suspensions, suitable for densitometric measurements of growth, which are rapid and convenient. Organisms that grow as mycelial mats, or flakes, leaving a clear culture fluid, can usually be weighed easily.

Marine organisms are superbly fitted for the study of the nutritional role of electrolytes since they require such electrolytes unusually intensely. They are also well fitted for studies of the mode of capture of trace elements. A marine *Chlamydomonas* having a high temperature optimum, the brine flagellate *Dunaliella salina*, and a variety of obligately halophilic microorganisms (requiring a high concentration of salt) are being studied together with that classical object of marine microbiological researches, the diatom *Nitzschia closterium*. It has been found that EDTA can be used as a metal carrier for all these organisms and for several marine bacteria, and the freedom from precipitation,[16] and the excellent growth obtained

[16] Attempts in the laboratory to duplicate seawater have ignored the fact that seawater is a saturated solution in contact with a significant solid phase—the shore and bottom—and that this equilibrium has been maintained for eons. It would appear preferable from the biological point of view, in studying marine organisms, not to attempt to construct a literal synthetic seawater, but rather to seek a functional equivalent of seawater which would embody quantitative knowledge of the essential constituents of seawater for marine organisms. Such an equivalent may be quite different from seawater in actual total composition.

(Vishniac, W., L. Provasoli, S. H. Hutner, and M. M. Weber, unpublished) hints that a great wealth of marine organisms may be tapped by the protistologist and biochemist seeking better objects for the elucidation of fundamental biological principles.

Further Notes on Apparatus and Materials

It may be said with considerable justice that we live in the borosilicate era of nutritional research. Nearly all laboratory-scale experiments are conducted with chemicals prepared with glass or glass-lined equipment. It would appear ill-advised to take great pains to prepare pure distilled water with the aid of quartz or platinum condensers without devoting comparable attention to the chemicals and culture vessels used. Pyrex glass is not immune from chemical attack, particularly by alkaline solutions. Reagent grade corrosive chemicals are apt to be heavily contaminated with the characteristic components of borosilicate glasses (68).

1. *Distilled water*

Since a supply of pure distilled water is needed not only for direct use in culture media but as a vehicle for purification of chemicals, an efficient still is essential. By far the most efficient stills are those in which the water used for condensation is recycled into the boiling chamber. It is safe to distill only once if an efficient splash trap—one which effectively stops liquid droplets without interposing too great a barrier to the passage of steam—is used. An investigation of the performance of splash traps is needed. One method of determining the extent of entrainment might be to add a vitamin stable to boiling, such as *p*-aminobenzoic acid, and then determine by assay with *Acetobacter suboxydans* whether any of the vitamin was carried over into the distillate. This type of test can be made exceedingly sensitive—at least to one part in twenty million.

The other critical point of the still is the condenser. Exposed as it is to the corrosive action of steam, it should be made of an inert material. Quartz has traditionally been used here. The advent of the new thermostable and exceedingly inert plastic, polytetrafluoroethylene, may provide an inexpensive and well-nigh ideal substitute for quartz.

With provision of an efficient splash trap, it becomes entirely feasible to have the boiling chamber made of metal. Such stills have been designed in England (69) and are commercially available.

2. *Chemical apparatus and culture vessels*

Several of the procedures previously described for purification of nutrient chemicals are likely to involve attack on the glass equipment used. This is a serious experimental hazard. The preparation of phosphoric acid from phosphorus oxychloride with neutralization by ammonia would yield a product of questionable purity if carried out in ordinary borosilicate apparatus. Quartz or polytetrafluoroethylene or other inert plastics should therefore be used for critical apparatus: e.g., the distilling head of the fractioning column, the condenser, and the receiving vessel. Culture vessels and bottles for storage of nutrient solutions are also critically important. Here thermostable translucent plastic containers would be of the utmost value.

One of the most powerful deterrents to potential investigators of the nutritional role of trace elements is the tradition of drudgery and of acid-laden atmospheres in which the work is set. Tradition has it that glassware must be cleaned with chromic-sulfuric acid, rinsed with distilled nitric or hydrochloric acid, and rinsed again with glass-distilled water. Few biological laboratories in the past were equipped with the corrosion-resistant fixtures and ventilating systems needed for the safe handling of the large numbers of culture vessels called for in this work. The advent of cleaning mixtures based on synthetic detergents is a decided improvement. The inclusion in cleaning mixtures of a small amount of a stable solubilizing chelating agent such as the sodium salt of ethylenediamine tetraacetic acid, enough to dissolve the precipitates of iron and calcium phosphates which so easily form in culture vessels, should represent another improvement.

CONCLUSION

In tracing the metabolism of cobalt as it comes to light in the chain of biosyntheses extending from vitamin B_{12} to desoxyribose nucleic acid and the gene, an effort has been made to show how the cobalt problem may serve as the type for a whole series of investigations in the metabolic role of other trace elements. The adduced parallel case

of iron and hemin further illustrates the potentialities of this line of research. The application of such investigations may range from a better understanding of the mechanism of the cell and the nature of viruses and the gene to the development of sounder agricultural and stock-raising practices. These aims, however, can be fulfilled only by extensive further investigation of an essentially infant field, with critical and meticulous attention devoted to laboratory technique.

ACKNOWLEDGMENTS

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ADDENDUM: SYNTHETIC MEDIA

All concentrations expressed as weight per 100 ml. of final medium

I. *All-purpose mineral base, especially for pseudomonads and enrichment cultures*

EDTA	0.05 g.	B	2.0 mg.
K_2HPO_4	0.02 g.	Mn	1.4 mg.
$MgSO_4 . 7H_2O$	0.08 g.	Fe	1.0 mg.
NH_4Cl	0.02 g.	Mo	0.6 mg.
Zn	5.0 mg.	Cu	0.4 mg.
Ca	2.0 mg.	Co	0.4 mg.

Bring to pH 6.6–6.9 with KOH; distilled water to 100 ml. A double-strength base will not precipitate. For *Pseudomonas aeruginosa* replace the NH_4Cl with 4% NH_4NO_3, and add glycerol 2.0% as a substrate.

II. *Alkaline medium for fresh-water Chlamydomonas*

EDTA	0.02 g.	Zn	2.0 mg.
K_2HPO_4	0.025 g.	Fe	1.0 mg.
NH_4 acetate	0.1 g.	Mn	0.4 mg.
$MgSO_4 . 7H_2O$	0.05 g.	Mo	0.4 mg.
Glycine	0.2 g.	Cu	0.2 mg.
Ca	5.0 mg.	Co	0.05 mg.
B	2.0 mg.		

The pH of this medium may be adjusted with ammonia vapor.

The double-strength will begin to precipitate at about pH 7.6. If KOH is used for neutralization, precipitation begins at about pH 8.0.

III. *Medium for marine Chlamydomonas*

EDTA	0.05 g.	Zn	3.0 mg.
K_2HPO_4	0.02 g.	Ca (as NO_3^-)	2.5 mg.
$MgSO_4 . 7H_2O$	0.25 g.	Mn	1.0 mg.
Glycine	0.25 g.	Mo	1.0 mg.
K acetate	0.2 g.	Fe	0.6 mg.
NaCl	0.25–4.0 g.	Cu	0.5 mg.
B	4.0 mg.		

pH adjusted to 7.0 with KOH.

Remarks: as seen, these media admit of wide variations in concentration of nutrients and pH. The 0.05% EDTA is usually replaceable with about 0.2% Na_3 citrate $. 2H_2O$, or 0.25% K_3 citrate $. H_2O$. To minimize risk of toxicity it has on occasion been found advisable to lower the trace elements to a fifth of the values given, while using K_3 citrate $. H_2O$ 0.05% instead of EDTA. The relatively concentrated EDTA media shown above allow heavy growth of certain organisms, but the high initial concentrations of potentially toxic elements such as copper and manganese may be too great a hazard for delicate forms.

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THE CHEMICAL COMBINATION OF INSULIN WITH MUSCLE AND ITS HORMONAL REGULATION

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THE mammalian hormones are characterized mainly by their effect on metabolic activity. The continued advance in the fields of hormonal and metabolic chemistry has stimulated the biochemist to attempt to describe the action of a hormone in terms of its effect upon one or perhaps two individual chemical or physical mechanisms. But metabolic systems are exceedingly complex and in addition hormones are known to act not singly but in complex interplay with each other. It is hardly surprising, then, to note that there is no hormone whose effects on mammalian metabolic processes can be described in terms of a single chemical or physical mechanism.

Insulin is of particular interest because it is obvious that a complete understanding of the precise chemical action of insulin will contribute greatly to our understanding of the fundamental problem of diabetes. It is tempting to assume that insulin has some effect upon the complex of tissue enzymes which catalyze the multiple reaction concerned in the metabolism of carbohydrate, fat, and protein. Many of these enzymes can be obtained free from cells as homogenous systems. The relative simplicity of such systems invites experimentation for the localization of the action of insulin. For some years my colleagues, Drs. Haugaard, Marsh, and Hills,[1] and I have sought unsuccessfully to demonstrate an effect of insulin upon such systems. This difficulty of demonstrating insulin effects in cell-free systems is also apparent from a review of other experimental reports in the literature which reflects a diversity of opinions among investigators.

In sharp contrast to this equivocal situation with cell-free systems is the relatively easy demonstration of effects of insulin upon systems of intact cells. The intact animal, of course, reacts characteristically to injections of insulin. But in addition isolated surviving organs may be responsive. For example, the rat diaphragm, when equilibrated aerobically with glucose, utilizes more glucose and synthesizes more glycogen in the presence of insulin than a control diaphragm in the absence of insulin. It is possible, therefore, that insulin requires intact cellular morphology for its action. Moreover, it is natural to extend the principle of the necessity of structural integrity to include the hormone itself. In other words, insulin molecules do not exist within the cell in simple solution randomly oriented with respect to intra-cellular enzymes, but join in the pattern by combining with some cellular constituent producing thereby a fundamental change in the basic metabolic behavior of the cell. We[2] demonstrated that this was indeed the case by the following simple experiment: A hemidiaphragm from a normal rat was immersed for a brief period in a medium containing a small concentration of insulin. The diaphragm was then removed and washed twice with a large volume of plain medium. During this brief exposure to insulin the diaphragm had combined firmly with insulin. The chemically bound insulin exerted an effect upon the metabolic pattern of the diaphragm. This could be shown by equilibrating the diaphragm in a medium containing glucose but no added insulin. Invariably the hemidiaphragm which had been exposed to insulin for the brief preliminary period utilized more glucose and synthesized more glycogen than did its control mate. We interpret this experiment to mean: (1) that insulin combines rapidly with some constituent of the muscle cell; (2) that the combination cannot be dissociated by prolonged washing with an insulin-free medium; and (3) that the combined hormone exerts its customary effect upon the metabolic pattern of the tissue.

[1] Stadie, W. C., and Niels Haugaard, The hexokinase reaction in tissue extracts from normal and diabetic rats, *Jour. Biol. Chem.* **177**: 311–324, 1949.

[2] Stadie, W. C., Niels Haugaard, Julian B. Marsh, and A. Gorman Hills, The chemical combination of insulin with muscle (diaphragm) of normal rat, *Amer. Jour. Med. Sci.* **218**: 265–274, 1949; Hormonal influences on the chemical combination of insulin with rat muscle (diaphragm), *ibid.*, 275–280.

TABLE 1

STANDARD TECHNIQUE FOR MEASURING COMBINATION OF INSULIN WITH RAT DIAPHRAGM

Test and control hemidiaphragms treated alike except for insulin in fixation period.

I—FIXATION PERIOD	II—WASHING PERIOD
1 minute; Insulin 0.1 unit/ml.	Twice with 25 ml. of medium
III—ASSAY PERIOD	IV—ANALYSIS
90 minutes; with 0.2% glucose	Measure of insulin combined is "insulin effect" = extra glycogen synthesized by test hemidiaphragm

Representative experiments will be understood by reference to a diagram (table 1) which gives the pattern of most of the experiments. In all cases there is an initial period termed the "Fixation period" during which the diaphragm freshly removed from the rat is exposed to insulin for a given period of time at a given concentration. Our standard method for purposes of intercomparison defines the fixation period as of one minute duration with the insulin at 0.1 unit per cc. The diaphragm is then washed twice in 25 cc. of medium. In the third or assay period the diaphragm is equilibrated for a period of 90 minutes in a glucose containing solution. Finally the diaphragm is analyzed for glycogen. The glycogen synthesized by the insulinized diaphragm was compared to that synthesized by a control untreated with insulin. The difference is the "insulin effect" and measures the extent to which insulin has combined with the diaphragm during the preliminary period.

The time required for the combination of insulin with the diaphragm may be quite brief (table 2).

TABLE 2

COMBINATION OF INSULIN (0.1 unit/ml.) WITH SURVIVING HEMIDIAPHRAGM OF RAT

Time of exposure to insulin	Insulin effect on glycogen synthesis (gluc. μ equiv./gm.)
10 seconds	2.8
1 minute	5.8
5 minutes	5.3
15 minutes	4.5
30 minutes	5.0

As little as 10 seconds may suffice to bind enough insulin to have a significant effect upon glycogen synthesis.

The striking thing here is that increasing the time of fixation from 1 minute to 30 minutes at a constant concentration of insulin does not give rise to any increase in the amount of insulin combined. In other words, it is possible to infer that a state of equilibrium between insulin and tissue had occurred at approximately 1 minute. This, we believe, is the first demonstration of a definite chemical reaction of insulin which is finite in time. When the concentration of insulin is increased during the one minute of fixation time, the insulin bound is increased and tends to reach a saturation level as indicated by the data in figure 1.

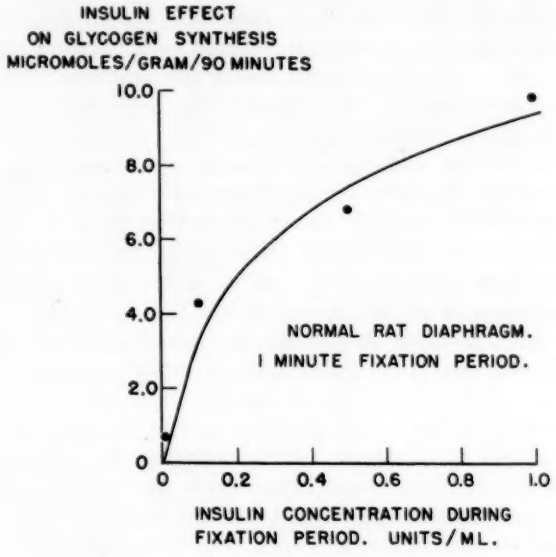

FIG. 1.

Several experiments were done to demonstrate that the combined insulin could not be dissociated by prolonged washing of the tissue. The one shown in figure 2 is the most interesting of these.

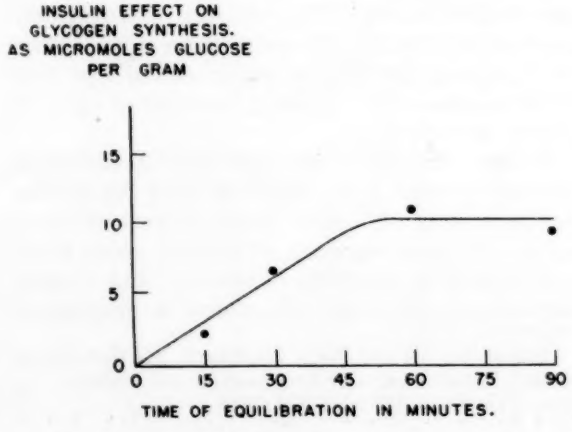

FIG. 2.

Following the fixation and washing period the diaphragms were assayed in the glucose system for periods of 15, 30, 60, and 90 minutes respectively. Each point on the diagram is the mean of 4 experiments. We see that the effects of the chemically bound insulin continue unabated for 60 minutes. In other words, there is no evidence that any dissociation of the combined insulin occurred during the time; otherwise the insulin effect would have fallen off.

It is interesting to make some sort of an estimate of how much insulin is combined with the diaphragm. Only the crudest approximations can be made and then only on the basis of some reasonable hypothesis. If the site of the attachment is some cell constituent the insulin must first traverse the covering thoracic or abdominal mesothelium. The rate of diffusion through these layers during the short exposure to insulin would then limit the amount which would be chemically bound. We assume that the muscle substance offers no more impediment to the passage of insulin than does water. We can use the fact that a significant effect was obtained when the concentration of insulin was 0.1 unit per cc. and the time of exposure 10 seconds. This gives a value for the amount of insulin diffusing into the diaphragm of 0.00001 of a unit (*ca.* 0.4 microgram) per gram of muscle. This estimate is, of course, a maximum and the real value may be much less. It is possible, however, to conclude that minute amounts of insulin combine with remarkable rapidity with tissue structures to produce significant changes in tissue metabolism.

The demonstration of this direct chemical combination of insulin with tissue component raises many interesting questions, foremost among them being whether the combination is a prerequisite for the physiological action of insulin. It is well known since the classical experiments of Houssay, and Long and Lukens that an interplay of hormonal factors from the pituitary, the adrenals and the pancreas is concerned in the phenomenon of diabetes. We have observed in preliminary experiments that the chemical combination of insulin with the diaphragm is similarly controlled. When the reaction was studied in diabetic rats (table 3) it was found that the diaphragms were refractory and combined to a less extent or not at all with insulin. The nature of the factors which bring about this refractory state in the diabetic muscle remains to be determined. We were unable to demonstrate that adrenalectomy or hypothesectomy of the rat produced any effect (table 4). However, when preparations of beef anterior pituitary gland were used, definite alterations of this property of the muscle were demonstrated. A crude saline extract injected 20 hours before the experiment was found to abolish completely the ability of the diaphragms to combine with insulin. Fractionation of these crude preparations using the method described by Wilhelmi [3] and his co-workers demonstrated that the inhibitory factor consistently followed the procedures outlined for the isolation of the growth hormone. Through the kindness of Dr. Wilhelmi we were able to test the activity of several growth hormones in a high state of purity including some crystalline preparations stated to be the purest obtainable at that time. In all cases the injection of approximately 0.4 mg. 24 hours antecedent to the experiment was sufficient to destroy the ability of the rat diaphragm to combine with insulin.

These experiments indicate that the rapid and stable combination of insulin in rat diaphragm can be influenced by a variety of factors which are

TABLE 3

EFFECT OF ENDOCRINE ABNORMALITIES ON THE ABILITY OF THE ISOLATED DIAPHRAGM OF THE RAT TO COMBINE WITH INSULIN IN VITRO

Experimental condition	No. of rats	Insulin effect on glycogen synthesis (gluc. μ equiv./gm.)
Normal	45	5.8 ± 0.5
Mild diabetes	10	2.9 ± 1.2*
Severe diabetes	7	-0.1 ± 1.3*
Adrenalectomized	5	4.9 ± 1.5
Hypophysectomized	8	6.1 ± 1.5

* Significantly different from control normals.

TABLE 4

EFFECT OF 20 HOUR ANTECEDENT INJECTION OF ANTERIOR PITUITARY PREPARATIONS ON THE ABILITY OF THE ISOLATED RAT DIAPHRAGM TO COMBINE WITH INSULIN IN VITRO

Preparation injected	No. of rats	Insulin effect on glycogen synthesis (gluc. μ equiv./gm.)
Normal controls	45	5.8 ± 0.5
Crude beef APE	12	1.5 ± 0.8**
Fraction D*	6	9.5 ± 1.5
Crystalline growth hormone	12	-0.1 ± 2.0**

* Contains no growth hormone.
** Significantly different from normal controls.

[3] Wilhelmi, A. E., J. B. Fishman, and J. A. Russell, A new preparation of crystalline anterior pituitary growth hormone, *Jour. Biol Chem.* **176**: 735, 1948.

probably hormonal in origin. The diabetic state induced by alloxan is one such condition.

Our experiments indicate that a factor in the pituitary, presumably the growth hormone, is also concerned with the regulation of this reaction.

It is obvious that our experiments have raised more questions than they answer. However, we feel that a new concept has been introduced into the discussion of insulin action. It is that insulin combines in some fashion, perhaps with a mosaic of enzymes, or perhaps with cellular surface structures. In this way the oriented enzymic patterns characteristic of cellular structure are extended to include the hormone and the new structure results in changes in the metabolic processes of the cell characteristic of the action of the hormone in question. The experiments also suggest a mechanism by which the interaction of hormones is attained, namely, by competition for some site of cellular structures for combination with insulin. The extent to which these new concepts will further develop our understanding of the phenomenon of diabetes and the action of insulin is a matter for continued experimentation.

DEVELOPMENT OF CONTRACTILITY [1]

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(*Read November 5, 1949*)

THE phenomena associated with muscular contraction have long been a focal point of scientific endeavor. The problems are so numerous and so attractive that workers in many branches of science have been intrigued not only by multiple approaches to an explanation of contractility but also by the apparent simplicity of this dynamic process, which, upon deeper study, yields a complexity for which so far no suitable explanation has been reached. A truly colossal volume of information involving all the branches of science has been obtained.

The earliest work was that of the anatomist who became acquainted with the macroscopic picture of the muscles, naming them and diagramming in a gross way the action of the various components of the system. Of the many workers I shall refer only to Leonardo (1452–1519), Vesalius (1549), and Borelli (1681). The first two dealt dominantly with form, secondarily with function; but Borelli had in mind primarily the functional aspect of the muscles. Borelli was interested in the mechanisms by which motion was produced. His observations emphasized the dynamics of muscle action by correlating the lever principle with the work done and thus showed the energy necessary for the work produced. This theoretical attitude is expressed in the trend of subsequent research. Leonardo approached the problem of muscle aesthetically, Vesalius practically, Borelli with the application of physical principles.

Modern research shows much the same trend with the anatomists working at the ultrastructural level. W. J. Schmidt (1937), F. O. Schmitt (1939), the physiologists Helmholtz (1848), and Hill (1949) have worked on the energy constants while the chemists are now investigating in profusion the constitution of muscle and the way in which the chemical components react. Of these, myosin (von Muralt and Edsall, 1930) has been shown to be an essential part of the contractile machinery with potassium and adenosinetriphosphate as two other major components. Another substance has been secured by Szent Györgyi (1945–1949) termed actin which is closely associated with myosin in the contractile reaction, the compound being called actomyosin.

THE STUDY OF EMBRYONIC MOVEMENT

All of the work so far referred to has been performed on adult muscle. The first extensive study of embryonic motility was made by Coghill and Herrick (1915) in an investigation of the early behavior of the spotted salamander, Amblystoma. They were able to show the maturation

FIG. 1. The musculature of the body as shown in the Vesalius plates, 1549.

[1] Supported in part by a grant from the American Cancer Society through the National Research Council Committee on Growth, jointly administered by J. S. Nicholas and E. J. Boell.

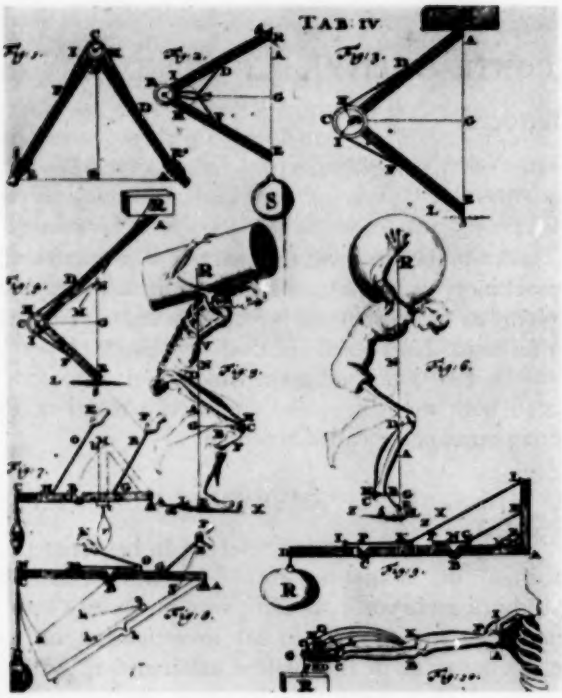

FIG. 2. The study of energy transfer. Borelli, 1681.

of the two components in the production of movement: the nerve and the muscle, and to correlate the changes during maturation with the development of the behavior pattern. Similar studies have been made on the genus homo by Minkowski (1928) and Hooker (1944), on sheep by Barcroft and Barron (1936), on the rat by Swenson (1928), Anguolo (1932), and Windle (1940). These works are dominantly related to the establishment of nerve connections and the influence of the developing nervous system upon the changing behavior pattern. There was no study of the embryonic muscle itself.

In order to study the changes which take place in the development of contractile tissues of rat embryos, a team of investigators was formed consisting of Professor E. J. Boell, Dr. S. C. Shen, Dr. Heinz Herrmann and myself. The studies here presented form a part of a group study in which the correlations so far established are given.

MYOGENESIS

In the twelfth and thirteenth day rat embryo, the myotomes which were discrete derivatives of the mesoderm begin to send down their ventrolateral extensions in the head and anterior body regions. These processes are the materials from which the later muscle bundles will form and at present represent by location rather than structure the later definitive muscles. These are the earliest muscles to form, anticipating those of the flank by two days and those of the posterior region by four to five days. There is here indicated a distinct anterior-posterior morphological progression which will likewise be noted in beginning functional conditions.

After the migration (fourteenth day) of the myotomic sheet, the cells (myoblasts) composing the sheet are aligned in parallel rows, the cells consisting of rather large nuclear units with comparatively little cytoplasm. At this time there is a superfluity of nuclear material which is reduced as the cell walls are broken down and the units extended longitudinally. Cells which are adjacent both proximally and distally become confluent. There is still considerable controversy about the way in which the myoblasts come together. In the rat the picture seems clear and so far, in skeletal (striated) muscle, no cases have been found in which there is formation of fibrils by attenuation of a cytoplasmic process. Most frequently this

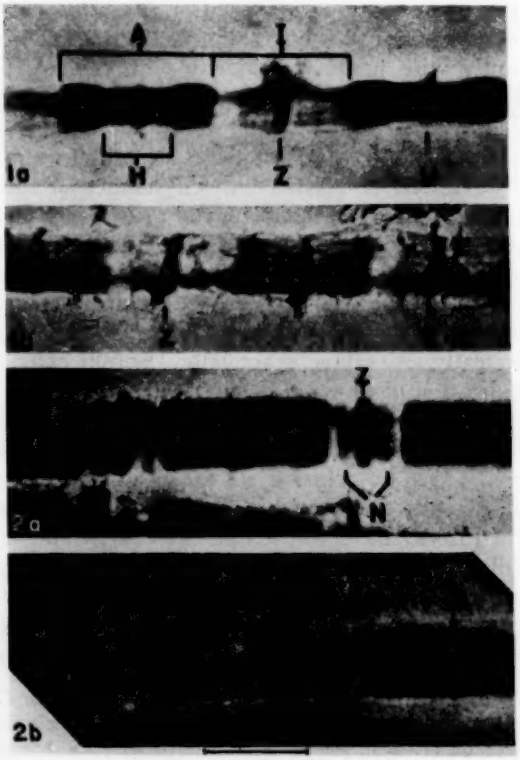

FIG. 3. The ultrastructure of the myofibril, Schmitt, F. O., 1949.

process begins at the region just inferior to the original somite border and progresses ventrally. The muscle fiber contains an excess of cytoplasm and granules with relatively reduced amount of nuclear material. At this time (fifteenth day), on the basis of evidence to be presented later, there is an internal molecular arrangement and the granules take up their positions in longitudinal strands forming fibrillae which are oriented within the fiber. During this process the nuclei are pressed peripherally and a separate set of granules begins to make its appearance as a horizontal striation in the muscle fiber (seventeenth day). This is the primitive embryonic striation which changes considerably before attaining the more differentiated condition of the adult muscle with the Z lines marking the repeat pattern of the contractile system. The relationship between the embryonic striation pattern and that of the adult is as yet not clear. It would appear from the studies so far completed that the adult pattern of striation is not completed until approximately ten days post partem and that it is formed either by interrelation or by interspersion of materials between the original embryonic striations. These new components are arranged in a repeat pattern of istropic and anisotropic substances.

Materials were prepared for the above study by spreading the mesoderm or the differentiating muscle upon a clean glass slide and tearing the material so that the muscle bundles were flat and straight. Samples were taken through all stages of development from the twelfth day through birth. The arrangement of the fibers was performed under the binocular dissecting scope using fairly high power (55 ×) in order to be sure that the fibers were not mechanically stretched.

The muscle tissue was fixed by adding formalin to the tissue film upon the slide and then either stained for the study of striations or left unstained for polarized light and phase contrast studies. In this way a double preparation was obtained and the study of one stage of development could be checked in both stained and unstained preparations and under a variety of microscopic methods.

The adult form of rat muscle is entirely similar to other mammalian forms and shows the same specializations which are common to striate muscle tissues (see fig. 4). A special study was made of the arrangement of the fibers and the early embryonic striations to test the form of their early genesis. Bernal (1939) suggested that the molecular arrangement of the fibers should logically be in the form of a spiral on the basis of Weed's (1936)

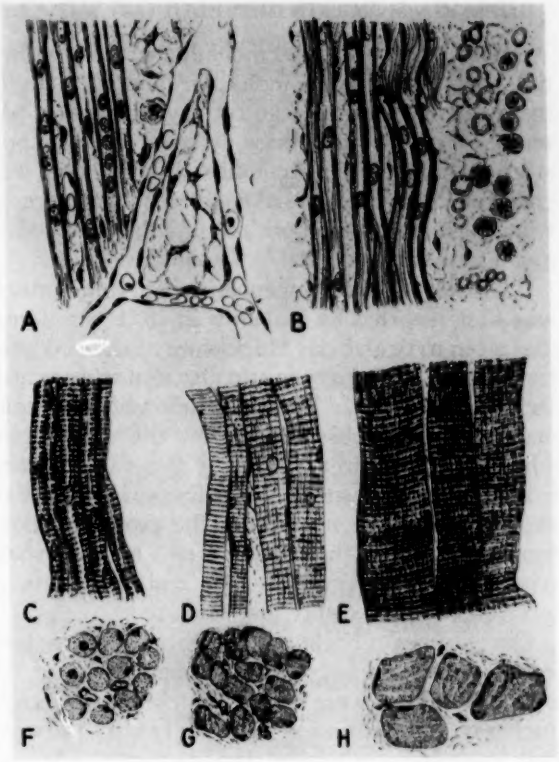

FIG. 4. Myogenesis in the human embryo, Patten, 1946. Histogenesis of skeletal muscle. (Camera lucida drawings, × 500.) A, From 32-mm. embryo. B, From 45-mm. embryo. C, From 200-mm. embryo. D, From the term fetus. E, From adult diaphragm. F–H, Transversely cut muscle fibers drawn from the same material and to the same scale as the longitudinal sections directly above them.

observations. This molecular arrangement, if present, should have a definite part in the arrangement of the fibrillae and upon the distortion of the striations. A muscle fiber as it is found in a muscle mass frequently is not in a single optical plane and if the longitudinal axis of the fiber is even slightly distorted the striations are apparently spiraled. This does not occur in our preparations where special precautions have been taken to avoid these conditions. The spiral effect is distinctly an artifact. This observation is in agreement with Astbury's (1947) x-ray diffraction studies, as well as those of Hall, Jackus, and Schmidt (1946) with the electron microscope (fig. 4). In neither the embryonic nor adult muscle is there any evidence of the development of the striations as either a continuous or a discontinuous spiral as advanced by Bernal (1939) and accepted by Szent Györgyi (1947).

BIREFRINGENCE IN DEVELOPING MUSCLE

Many studies have been conducted upon muscle to show the possible molecular arrangement by means of birefringence and x-ray diffraction patterns. The second of these does not directly apply since the studies so far conducted do not deal with embryonic structures. An excellent picture of what has been accomplished by this method is given in Astbury's (1947) Croonian lecture.

The study of birefringence in embryonic muscle was first reported by Valentin in 1871 who found that a ten to twelve day chick embryo showed positive muscle birefringence and also that it was capable of contracting. This was followed by Englemann's (1881) observations on the chick heart where he noted birefringence at two days of incubation which antedates any cardiac striation. Valentin should have noted that the cross striations were present and that they were of an embryonic type. Other observations on embryonic tissue birefringence are scanty and scattered.

Our studies (Nicholas and Shen) were performed on a carefully graded timed series of rat embryos. They were begun on developing muscle during its differentiation from the twelfth to twentieth days of intrauterine development. The unstained preparations which had been used in the study of myogenesis were employed for these observations. The graded muscle series was studied with a Zeiss polarizing microscope and the birefringence noted. The results of this study are shown in figure 5.

Beginning at the twelfth day and extending through the thirteenth, the myoblastic tissue shows a negative birefringence as do the myotomes giving rise to the myoblasts. This varies from Hobsen's (1941) findings on the chick embryo for he found birefringence in developing myotomes. On the fourteenth day the myoblasts and fibers become slightly positively birefringent. Up to this period it has not been possible to test regions other than those in the head, neck, and anterior limb regions since the tissues in the flank and posterior limb regions are distinctly retarded in structural development. With the fifteenth day, however, muscular contractions can be elicited in the anterior region and there is likewise the beginning of dorsoventral flexion bringing into play the abdominal musculature.

BIREFRINGENCE
EMBRYONIC RAT MUSCLE

	NECK	ABDOMEN	THIGH
12	− Bi	0	0
13	− Bi	0	0
14	+−Bi	0	0
15	+ Bi		− Bi
16	+ Bi	+ Bi	− Bi
17	+ Bi (ST?)	+ Bi	− Bi
18	++ Bi (ST)	+ Bi	+ Bi
19	++ Bi (ST)	++Bi (ST)	+ Bi (ST?)
20	+++Bi (ST)	++Bi (ST)	+ Bi (ST?)

NICHOLAS AND SHEN

FIG. 5.

At this time the anterior musculature displays definite positive birefringence; the flank musculature is weakly birefringent; while the posterior musculature is isotopic. There are no signs of striations; the muscle fibers are still chains of cells with parallel arrangement.

Movement and contractility occur after positive birefringence is established. The existence of double refraction requires an orderly arrangement of the molecules present in the developing muscle and this arrangement precedes contractility while both contractility and birefringence precede embryonic striation. The same order obtains regionally, for these same indices are formed first in the head and thorax, then in the abdomen and flank, and finally in the posterior parts of the body.

THE DEVELOPMENT OF MYOSIN

The myosin content of adult striated muscle is 10–12 per cent of its weight, the total protein present constituting 18–20 per cent of its weight. Of the proteins found in muscle only myosin shows double refraction. In embryonic muscle the myosin must develop along with the other muscle components beginning in a much lower concentration than that of the adult. In order to determine the protein content and the myosin development, chemical determinations were performed on a timed series of embryos from the 11th day of intrauterine development to the thirtieth day of post partem (Herrmann and Nicholas, 1948*a*).

In general five hundred milligrams of muscle tissue were used for each determination. In the earlier stages this demanded the treatment of about fifty embryos for a single determination. Newborn rats yield about two hundred milligrams; ten days after birth five hundred milligrams can be secured from one animal. Figure 6 shows the results of such determinations while figure 7 shows the conditions of precipitation by which the myosin and myosin subfractions were obtained. The re-

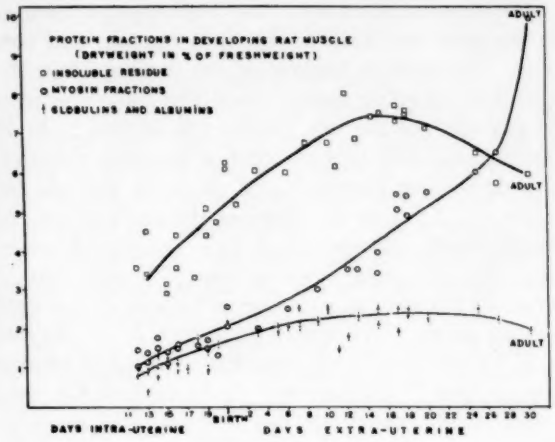

FIG. 6.

sults as a whole show that the myosin protein fractions in rat muscle increase at different and distinctive rates. The most marked changes occur during the twelfth to sixteenth days of development.

In order to test the myosin so secured from rat muscle, determinations were made of its capacity to liberate inorganic phosphate enzymatically from adenosinetriphosphate (Hermann and Nicholas, 1948*b*). This is known as apyrase determination and the result is shown in figure 7. The sudden rise on the sixteenth day correlating as it does with functional contractility at present must be regarded as coincidental rather than as a close correspondence between ATPase activity and functional differentiation. It may be the latter but it may also be linked with other general metabolic changes linked with variations in placental physiology from which it can not at present be dissociated.

Whatever the import of the above finding it permits us to support along with numerous other investigators Astbury's statement (1947: 321) "That myosin is both the working elastic mechanism in muscle and also a principal enzyme (adenosine triphosphatase) or includes a principal enzyme in its complex."

In table 1 R is the residue obtained after centrifugation of the whole homogenate which was homogenized in 10 cc of 0.5 *M* KCl with pH adjusted to 7.5. The supernate was adjusted to pH 5.7 by adding .04 *N* HCl and centrifuged, securing M_1 which contains most of the myosin. The supernate of this step is then diluted with 2 volumes of water and M_2 was obtained, S being the supernate.

TABLE 1

ATPASE ACTIVITY OF MUSCLE PROTEIN FRACTIONS CALCULATED PER MG. *N* OF WHOLE HOMOGENATE

Fraction	14 days gestation	19 days gestation	1–2 days post partem	9–10 days post partem
R	47	200	360	300
M_1	5	54	180	280
M_2	15	40	48	50
S	*66*	*250*	*170*	*220*
Σ	133	544	758	850
W	120	490	910	970
(W)	95	570	570	950

Σ = The sum of activities of single fractions per mg. *N* of the whole homogenate.
W = Whole homogenate value.
(W) = Values recalculated from previous data for activities of the whole homogenate corrected for increase in activity in the presence of KCl.

The four fractions develop their activity independently and at different rates. The activity of all fractions increases but the greatest increase in fractional as well as in relative activity is in the M_1 fraction (myosin) which increases fourteen fold in fractional and fifty-six fold in relative activity. The increase is due to the quantitative increase both in amount and activity of the M_1 fraction. R, M_1 and M_2 attain practically their entire increment from the fourteenth to nineteenth day of gestation when the most marked changes occur in the developing muscle. The activity secured from S increases but slightly over the entire period.

As can be seen from table 1 the amount of activity for the sum of the four fractions gives an exceedingly good agreement when checked against the previous determinations of whole homogenates. The M_1 fraction which develops its activity faster than the other fractions contributes only one third

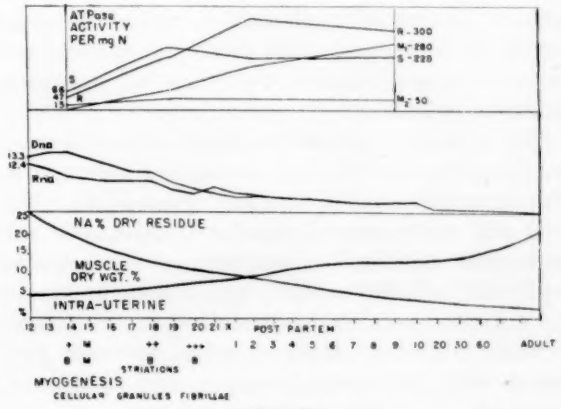

FIG. 7.

of the total activity. This contrasts with the previous views founded on adult muscle investigations where the myosin protein contributes practically the entire apyrase activity (Bailey, 1944). This accounts for the apparent discrepancy in our first experiments (1948*a*) with whole homogenates where the apyrase activity was much greater than was expected from the amount of myosin present.

CHANGES IN NUCLEIC ACID CONTENT OF DEVELOPING RAT MUSCLE

In studies referred to above (Herrmann and Nicholas, 1948*a*) the myosin development was determined and the changes in content noted. Szent Györgyi (1947) has shown that this protein can dissociate into two components, actin and myosin. Proteins similar to actomyosin in their solubility properties have been prepared from a variety of organs (Bensley, 1942, Brachet and Jeener, 1947). Mirsky (1943) emphasizes the similarity of the myosin and nucleoprotein solubilities and the same problem has been discussed by Brachet and Jeener (*loc. cit.*).

This question is a particularly trying one in embryonic material for, while in adult muscle the content of nucleoprotein is low and the myosin fraction contains an insignificant amount of this substance, the conditions are exactly reversed in embryonic muscle. Because of this large amounts of nucleoproteins are present in the extracts. Until, therefore, quantitative data could be obtained for the nucleoprotein content of developing muscle it was questionable whether the proteins secured in the myosin study were really myosin or were nucleoproteins.

To obtain the necessary data to determine whether we were dealing with myosin or nucleoprotein a series of determinations of the nucleoprotein content of whole muscle and of the separable fractions was made using the nucleic acid content as an index of the quantity of nucleoproteins present.

Embryonic rat mesoderm and muscle were studied from the twelfth day of gestation through its development to twenty-one days after birth. Nucleic acid was extracted with hot trichloracetic acid with colorimetric determinations of ribosenucleic acid (RNA) with orsinol, and desoxyribonucleic acid (DNA) with diphenylamine, Schneider (1945). Control standard solutions were prepared using commercial nucleic acid samples in which the phosphate content was known.

Ninety-nine samples of 500 mgs. each were used in the determination of the total nucleic acid content. Its value is highest at the earliest stage determined (twelve days) when the nucleic acid is 25 per cent of the dry residue; it decreases to 11 per cent at birth, to 4 per cent at weaning (twenty-one days post partem) and in adult muscle the value is 1.2 per cent. During the same phases of development, the per cent dry weight increases from 5.5 per cent (twelve days), 9 per cent at birth, 14 per cent at weaning and about 20 per cent for the adult. As shown in figure 7 the decline in NA content is faster than the increase in muscle dry weight. Throughout development the two nucleic acids contribute nearly equally to the total nucleic acid although the RNA concentration decreases slightly before DNA diminution begins (see fig. 7).

Since the fractions of the muscle proteins can be studied separately the muscle was divided chemically into the R, M_1, M_2 and S fractions and each examined for its nucleic acid content. The highest concentrations of DNA are in the R fraction of the developing muscle. As much as 23.3 per cent is present at the thirteen-fourteen day stage, decreasing to 7.9 per cent just after birth and to 6.2 per cent ten days after birth. The concentration of DNA in M_1 and S is virtually unchanged but there is a sharp rise in its value in M_2 on the tenth day. The RNA gives quite a different result for the highest concentrations are found in M_2 and relatively high values are found in S. M_1 has a lower value of RNA than any of the other fractions.

The experiments of this section show that the total nucleic acid present in developing muscle tissue decreases during development, that the M_1 fraction contains the smallest proportion of the nucleic acid content of any of the protein fractions and this fraction although not pure according to the standards of preparative protein chemistry is sufficiently depleted in nucleic acids to justify the belief that it contains almost the entire amount of the myosin proteins.

When these results are compared with the changes in developing muscle it is seen that there is a definite increase in the dry weight of the muscle protein while there is a decrease in the nucleic acid concentration. This shows an inverse correlation between nucleic acid concentration and protein formation in sharp distinction to Casperson's (1947) assumption that protein synthesis is correlated with high nucleic acid concentration.

THE BREAKING POINT OF DEVELOPING RAT MUSCLE

In a preliminary study by Boell, Nicholas, and Shen the breaking load was determined for the rat gastrocnemius muscle from the fifteenth day of gestation to forty days post partem. At the same time the maximum stretch of the muscle was measured up to the breaking point. The curve in figure 8 shows the load necessary to cause a break across the muscle bundle, the separate points indicate the maximum stretch attained.

At the younger stages the maximum stretch is hard to measure and the preparations are not always uniform but the measurements so far attained show that embryonic muscle on the seventeenth day of gestation has a maximum stretch of 180 per cent with a break load of 2.5 gms. The maximum stretch decreases sharply to 140 per cent at birth, rises to 160 per cent ten days post partem and then decreases gradually to 125 per cent at forty days post partem under 1,100 gms of breaking load.

These preliminary experiments with an entirely different approach from the highly chemical studies given above throw important light both upon the chemical relationship to the mechanics of muscle and also upon the integration of the contractile tissues and their envelope, the sarcolemma. The first relationship is shown in the great degree of stretch capacity in a practically undifferentiated muscle. The gastrocnemius of the seventeenth day gestation age shows no birefringence, has no striations, contains no fibrillae. There is no definitive sarcolemma and the connective tissues are much more nearly the consistency of a loose gel than a tissue. It is literally as similar to Weber's (1934) myosin threads as any structure which the embryo can produce.

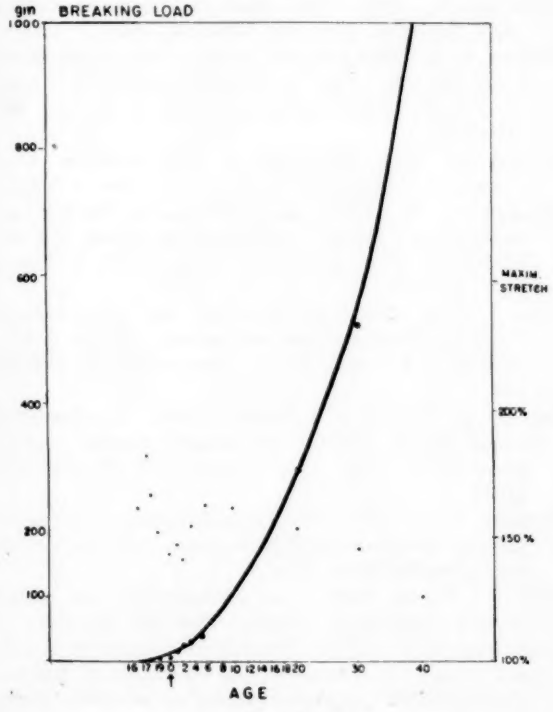

FIG. 8.

Ramsey and Street (1940) working with single muscle fibers found that the sarcolemma withstood the same tension as the fiber, for the muscle substance could be broken and the tension remained uniform. Our experiments are more nearly comparable with Weber's (1934) artificial myosin threads containing 20 per cent protein which reacted against stress with the same tension which would be obtained from muscle of similar cross section. In both cases there is no sarcolemma but there is the resistance to stretch. It might easily be true that the sarcolemma obscures some of the contractile properties of the contained muscle fiber in the older isolated fibers such as Ramsey and Street used. It may even act as a mechanical inhibitor in damping the contractile reaction at the same time exhibiting its own reaction to stress relations by taking over the properties of an envelope or sac containing a fluid.

CONCLUSION

The survey of the work upon developing rat muscle shows in an interesting way the differences between this type of study and those conducted upon the tissues of older animals. Differentiation and growth are parallel processes which at times are competitive within the embryo. The quantitative estimation of the development of specific chemical entities and their correlation with the morphological and physiological changes show not only the items of chemical interest but also the part played by them in these two important processes in embryonic development. In the case of muscle development our work has shown clearly that myosin, a necessary component of the contractile reaction, develops within the muscle complex in association with other proteins which are chemically separable and which show constitutions with different enzymatic activities. The ratio of increase and regression occurring in these various fractions can be determined with a fair degree of

accuracy as shown by the agreement of values in determinations made by a variety of methods.

In addition to establishing the import of myosin in the developmental picture, the studies have given us additional information on the other factors peculiar to developmental contractility such as the contribution of the other chemically obtained fractions which are not present in adult tissue. This extends considerably our knowledge of the adaptibility of the embryonic muscle which contains not only a functional myosin component but also three other separable parts each of which contributes to a reaction heretofore thought to be limited to myosin alone.

The relationship of nucleoproteins to the chemical reaction is quite at variance with Casperson's widely accepted view that during protein synthesis the nucleic acid content is increased. In our findings the muscle is increasing in size, strength (as indicated by its breaking point), and in protein content, but the nucleic acid is constantly decreasing during this entire period of differentiation. In the embryo the nucleic acid behavior in contractile tissue is in inverse correlation to Casperson's concept.

The separability by chemical procedure of the nucleoproteins from the myosin proteins shows that the analysis of the myosin activity is not affected by the presence of large amounts of nucleoproteins for the residue (R) which contains a large proportion of the nucleoproteins shows an enzymatic effect equivalent to that shown by the myosin fraction which contains the least amount of these proteins. The last observations given above show the similarity of the embryonic muscle reactions to tension which resemble those obtained with artificial myosin fibers. These are important first steps in the understanding of developing contractility.

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